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ASPECTS OF TRANSFERABILITY IN SECOND LANGUAGE ACQUISITION

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A note on the text

Chapters One and Two have been specially written for this thesis. Chapter One contains some material originally published elsewhere. Chapters Three, Four, Five, Six and Eight were published in 1974, 1977, 1982, 1986 and 1985 respectively. Chapter Seven has been submitted for publication.

As far as the published articles are concerned, no attempt has been made to 'improve' the content in the light of subsequent research or changes in thinking. Consequently there is a certain amount of inevitable cross-referencing and summarizing of previous work. What changes there are in the body of the text have been made purely with an eye to readability. This has entailed some deletion, addition, rewriting and re-ordering. The aim throughout has been to produce a less dense and more clearly formulated account of my work while making the smallest possible number of amendments to the original texts.

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1 CROSSLINGUISTIC INFLUENCE A REVIEW[1]

1 0 Introduction

This thesis sets out to describe and delimit an essentially experimental approach to the study of the role of the native language (L1) in second language acquisition The goal of this approach has been a contribution towards the establishment of the conditions under which the native language (L1) will play a role in the development of the interlanguage (IL), particularly with regard to patterns of lexicalisation I take it for granted that the evidence for L1 influence is overwhelming, that the forms such influence takes are interestingly diverse, and that such influence is not qualitatively restricted to particular types of learning context Consequently, any theory of second language acquisition must subsume a theory of L1 influence (cf Wode, 1984, 1986)

This chapter is intended to provide a broad survey of some of the issues past and present which are important to our understanding of the phenomenon variously known as transfer, interference, or, as it will be called here, crosslinguistic influence It is divided into six sections as follows Section 1 discusses terminology, Section 2 deals with Dulay and Burt's arguments against a role for the L1, Section 3 considers the question of prediction, Section 4 is a selective review of the evidence for L1 influence, methodological problems as illustrated in the study of relative clauses are the topic of Section 5 Section 6 forms the conclusion to the chapter

1 1 A question of terminology

The moment one begins to discuss the role of the L1 in second language acquisition, it becomes necessary to sort out the question of the appropriate terminology This is because there are a number of terms used in second language acquisition with similar or partially overlapping meanings, all of which have to do with the interaction of two different language systems within the learner It is important to clarify this issue because by doing so one may

also clarify the issues motivating the choice of terminology One solution to the problem of potential confusion has been proposed by Sharwood Smith and Kellerman (1986), who introduce the superordinate term crosslinguistic influence

There are two main points which need to be made with respect to the term 'crosslinguistic influence' (CLI) First, it is relatively new (cf Sharwood Smith, 1983, Kellerman, 1984, Sharwood Smith and Kellerman, 1986), second, it is theory-neutral, allowing one to subsume under one heading such phenomena as 'transfer', 'interference', 'avoidance', 'borrowing' and L2-related aspects of language loss, and thus permitting discussion of the similarities and differences between these phenomena

As is evident from the literature, terms like 'transfer' and 'interference' are still current, though some scholars have felt that they belong properly to the parlance of laboratory learning experiments and should thus not be employed elsewhere because "they may perhaps constrain one's freedom of thinking' (Corder, 1983 86, cf James, 1977 154) It is certainly true that whatever its traditional associations, the term 'transfer , if only by virtue of its everyday meaning, does not permit a discussion of less obvious modes of crosslinguistic influence resulting in 'avoidance' by learners of L2 structures radically different from their L1 equivalents (Schachter, 1974) or leading to differential rates in the acquisition of certain L2 structures such as negation because of the existence of a developmental stage in the L2 which coincides formally with the mature L1 structure (see e g Schumann, 1982) This is indeed an unwarranted check on our thinking The use of the term 'transfer' should be restricted to those processes that lead to the incorporation of elements from one language into another Retention of the term would also prevent an undue proliferation of terminologies, thus reducing the risk of misunderstandings (cf the problems of interpretation revolving round such terms as 'markedness', 'universals', 'learning', 'acquisition') Finally, when not being used specifically in the context of behaviourist theory, terms like 'interference and 'facilitation', with their respective negative and positive connotations, are best abandoned altogether there is simply no reason to entertain value judgements

concerning psycholinguistic processes that are being investigated in their own right The teacher or layman may view the mixing of different languages in learner performance as an inevitable obstacle to progress or as a regrettable (if sometimes humorous) fall from grace there is no reason why the researcher should think so as well

'Transfer' and 'interference' have by convention (if not by design) generally been understood to relate only to the effect of L1 on L2 However, as Sharwood Smith (1983) and Py (1986), amongst others, have recently shown (and as Weinreich, 1953 and Haugen, 1956 showed some thirty years ago), there is also good evidence for the effects of L2 on L1 There is thus a need to find ad hoc labels to describe changes in L1 competence caused by contact with the L2 The situation is further complicated by the ambiguity of the term 'language loss', which has been used both to refer to the forgetting of a once-learned second language as well as to the loss of L1 competence Here too the term crosslinguistic influence' can be used without further ado to describe the processes involved whatever the direction of the influence Similarly, the evidence is that second languages can influence each other in their learning (Ringbom and Palmberg, 1976, Ringbom, 1978, 1985, Sjoholm, 1983, Chumbow, 1981, Polio, 1985)

A further advantage to be gained by the use of the term 'crosslinguistic influence' is that we can conveniently subsume terms like 'avoidance' (Schachter, 1974, Kleinmann, 1977, 1978) under it It is now established beyond doubt that the L1 can have a constraining role in the L2 production of learners - that is to say that the perception of differences between L1 and L2 by learners may effectively prevent production of L2 structures (just as the perception of similarities may lead to what Kean (1986) has called 'short-sighted transfer') Avoidance in this sense presupposes that the learner is to some extent aware of what the target structure must be like, at least enough to know that it is <u>not</u> like the L1 and therefore difficult Of course, whether the learner jumps in at the deep end and takes the risk of making a mistake or prefers to play safe is a question that a theory of crosslinguistic influence is not required to answer It is enough that such difficulties can be

located and anticipated The ensuing linguistic behaviour - the avoidance or the risk-taking - is still the outcome of crosslinguistic influence [2] Since Lado claimed to be able to predict difficulty in learning (Lado, 1957, Preface), and avoidance is just as much an indicator of learning difficulty as error (errors of omission and commission, we might say, to underline the point), the use of the term CLI permits us to ascribe a role to the L1 (or other source language) in the determination of difficulty without the need to distinguish between 'interference error' and avoidance Such differences as there are do not relate to the omission/commission distinction but rather to the problem of awareness Avoidance implies problematicity, and therefore some awareness of what the L2 target is like (or at least what it is not like) If one can demonstrate avoidance, one can claim awareness on the part of the learner, on the other hand, errors cannot be taken as unambiguous signs of awareness of problematicity It would be just as necessary to demonstrate risk-taking on the part of the learner Difficulty and error need not co-incide if difficulty is to be seen as something actually experienced by the learner, and not merely attributed to him. When we say that Structure X is difficult for Dutch learners, we are really saying that Dutch learners typically make errors when using it, despite the presence of the target in the input. Very often this reduces to a teaching problem - how can we teach Structure X in such a way that input becomes intake? Part of the teaching task is to make learners conscious of the problem that the L2 poses. This is the confrontation phase If learners are not aware of the difference between L1 and L2 then 'difficulty' becomes a metaphor for failure to capitalize on input Logically, then, any structure present in the input which is not immediately converted into intake must become difficult, whether it is different from L1 or not This will require that L1-equivalent structures which are 'ignored' by learners are also 'difficult' There is good evidence that this is indeed sometimes the case (see Chapters 4 and 5 of this thesis for discussion), and it weakens the 'difficulty/difference' argument considerably.

The growing evidence demonstrating that structural identity is not a sufficient condition for transfer to occur (e g Andersen,

1983; Kellerman, 1983 and this thesis, Zobl, 1983) and that therefore there must be L1-related constraints on the form of interlanguages in addition to natural acquisitional principles, also constitutes an argument for a change in terminology. In such cases, L1 influence is no less real, if less tangible, in conditioning what learners will *not* do than in those cases where 'interference errors' appear or where there is avoidance

Lastly in this list of advantages accruing to a change in terminology, the use of the term 'crosslinguistic influence' need not be restricted to the study of second or foreign language acquisition. So much is clear from Wode's plea (Wode, 1986) for research into CLI to be extended to many kinds of language contact situation (e g pidginising contexts, and the relearning of previously known languages, cf. Andersen, 1984a). Wode is concerned with the linguistic similarities that link the various contact situations he studies, similarities which show that, despite major differences from the sociolinguistic standpoint, the underlying psycholinguistic processes at work in CLI are essentially the same in all cases.

1.2 Dulay and Burt

Non-specialists have always tended to assume that the L1 is a major factor in second language acquisition and performance. And certainly until relatively recently, this assumption was well supported in the literature. The key date in this respect is usually taken to be 1957, since that year marked the publication of Lado's *Linguistics Across Cultures*, a work which in its time exercised a profound influence on the field. Lado's fundamental claim is known as the Contrastive Analysis Hypothesis. It views acquisition as a process of overcoming established L1 habits and states that difficulty in learning foreign languages could be predicted via a thorough comparison of the native and the target language. Where there were structural differences between the two languages there would be learning difficulties. Where the two languages were structurally equivalent there would be facilitation of learning Pedagogical materials based on the linguistic

descriptions could then be written specifically to drill the differences, and thus help to prevent errors being made by learners This is not the place to debate the rise and fall of the Contrastive Analysis Hypothesis (CAH), since it has already been done many times (see e.g. Aarts, 1982 and van Els, Bongaerts, Extra, van Os, and Janssen-van Dieten, 1984 for thorough reviews) However, since in the intervening years Lado's views and those of his successors have been largely discredited, the time is perhaps ripe for a few comments directed at his best-known and most virulent detractors.

One of the most consistent anti-crosslinguistic stances in the second language acquisition literature is that associated with Dulay and Burt. In their recent book, *Language Two* (Dulay, Burt and Krashen, 1982), Chapter 5 is devoted to the question of "The Role of the First Language". Since this chapter would seem to be the most recent distillation of their ideas, it is as well to devote some time to them.[3]

Dulay and Burt essentially base their rejection of the importance of native language influence on the evidence of universal (i.e common) orders of development and error patterns as manifested in the performance of learners of English with markedly different mother tongues (cf. Bailey, Madden and Krashen, 1974 for a good example of this sort of argumentation). The fact that second language learners with differing language backgrounds follow similar developmental paths and that the observed orders are not entirely dissimilar to orders observed in first language acquirers permits reinterpretation of apparent cases of transfer as cases of (over)regularisation of material in the L2 input (see e.g. Dulay et al., 1982: 211-14). In an influential article on the interpretation of errors (Dulay and Burt, 1972), Dulay and Burt argue that a mere equivalence between two linguistic products, a learner utterance and the equivalent native language utterance, is not enough to justify the conclusion that the learner has resorted to the psycholinguistic process of L1 transfer. the fact that mother tongue acquirers produce the same kind of non-target form is further reason to avoid this naive confusion of product and process Finally, the evidence derived from observing common developmental sequences (referred to above) would seem to provide the

justification for rejecting transfer as a significant factor in second language acquisition and performance.

It should be said immediately that Dulay and Burt are only concerned with the question of crosslinguistic influence in phonology, morphology and syntax. (Though they themselves have not conducted research into L2 phonology, they readily admit that this is the one area where CLI is common, e.g. Dulay et al., 1982.96) Lexis, semantics, discourse and pragmatics, all of which are also susceptible to CLI, are not referred to by the authors

Dulay and Burt challenge the CAH on several counts (Dulay et al., 1982:97). Amongst the most important are·

a) the CAH is based on an inadequate psycholinguistic theory (behaviourism)
b) appropriation of data from bilingual contact studies in support of the CAH is invalid
c) the majority of learners' errors do not reflect the L1
d) errors are made even where L1 and L2 are congruent in meaning and form
e) what evidence for CLI there is (bar phonological evidence) can be ascribed to environmental factors, e.g types of elicitation procedures or 'pressure to perform before one is ready'.

The first criticism, a), is fair and has been frequently discussed in the literature. However, as I try to show in Chapter Four, the existence of CLI need not be an embarrassment for a theory of language acquisition (though see Dulay and Burt's comments on attempts to subsume CLI under different theories of acquisition in Dulay and Burt, 1972 237).

Point b) concerns the misappropriation of research findings from bilingualism. In Dulay and Burt's famous 1972 attack on Lado (1957), which I also deal with in Chapter Four, we are led to believe that the latter's references to Weinreich and Haugen's work as the justification for the kind of Contrastive Analysis Hypothesis he was proposing were essentially misguided. I now believe Dulay and Burt to be substantially wrong in their criticism. Weinreich and Haugen, as Dulay and Burt are at pains to remind us, did not intend a kind of behaviourist interference where old habits got in

the way of attempts to acquire new ones, but rather a sociolinguistically motivated one under the control of the speaker or even the community. This sociolinguistic interference is to be seen in the borrowing and code-switching tendencies of individuals functioning as members of bilingual communities, not individuals in the foreign language classroom (cf James, 1980 8) The quotations from Weinreich and Haugen that are used to support this view suggest that this sociolinguistic interference is induced by familiarity[4] with more than one language, and not the lack of familiarity with the L2 characteristic of the foreign language learner (Weinreich, 1953·1), while it is the learner's L1 that is affected, not his L2 (Haugen, 1953.370) Again, in a Ladonian framework, we tend only to think of the L1 influencing the L2, and of such influence being beyond the control of the learner In the case of bilingual communities, on the other hand, switching between languages may be quite within the control of the speaker, depending on the nature of the interaction and the linguistic status of the interlocutor The presence of a fellow-bilingual will probably encourage switching, while the presence of a monolingual will not Code-switching is thus sociolinguistically motivated Dulay and Burt conclude that it was misleading of Lado to appeal to the work of Weinreich and Haugen to provide empirical respectability for his claims

However, although this account of Lado's argumentation is well-known, it is as well to point out that it does Lado a considerable disservice Firstly, Haugen (1953) and Weinreich (1953) are mentioned precisely once in the main body of Lado's text, on page 1 Haugen (1953) is listed in the Selected Bibliography (pp. 131, 137), and Haugen (1954) is also listed (p 126) Weinreich (1953) is not listed in the Selected Bibliography at all

Secondly, it is difficult to maintain that the term 'interference', at least as used by Haugen, is not the same as Lado's 'first language interference' Dulay and Burt (p 99) attempt to argue that Haugen's 'linguistic borrowing' is quite different from interference as conceived of by Lado This is quite correct since they are different phenomena, but it does not of necessity entail that Haugen did not believe in the notion of unconscious automatised transfer For instance, in the year prior

to the appearance of Lado (1957), Haugen (1956 40) wrote the following:

> We need to recognize that for certain items a linguistic overlapping is possible, such that we must assign them to more than one language at a time. This is the true interference between languages, and it might be better if the term were limited to such cases *It would not include, then, ... code switching ... nor the established loan ...* (italics added)

Also

> (The speaker) may also avoid interference in the strict sense by switching languages (1956 50)

Furthermore, there is a clear foreshadowing of Lado in the following passage from Haugen (1956 41)

> In theory it should be possible to predict the interferences that would occur if one had complete equivalent descriptions of the two languages and then compared these Such a comparison is an important tool in language learning, and most textbooks are more or less clearly based on this principle For pedagogical purposes it is essential to stress the differences between two languages, since these are the items that must be learned Failure to learn them results in interferences.

In this respect it is worth noting that Dulay and Burt (1974a.101) quote the crucial passage from Lado (1957:1) which refers to Weinreich and Haugen, but for obscure reasons miss out a key sentence in doing so (In Dulay et al , 1982, the quotation is dropped altogether) This passage, with the missing sentence in italics, is as follows

> A practical confirmation of the validity of our (Lado's) assumption has come from the work of linguists who study the effect of close contact between languages in bilingual situations *They report that many linguistic distortions heard among bilinguals correspond to describable differences in the languages involved* Extensive studies have been carried out by Haugen and Weinreich in this area

In this light, the Lado claim looks innocuous If it is indeed the case that many of the 'distortions' can be traced back to one or other of the languages in contact, and it is clear from the quotation that Lado was aware of this, then it is not correct to attack Lado for only considering the L1 as interfering source in the L2, the unidirectionality that Dulay and Burt find unacceptable (Dulay et al , 1982 99-100) Lado's claim seems merely to be that errors are, from the point of view of description, largely attributable to the interaction of language systems It is the case that Lado did not consider the possibility of language mixing to be sociolinguistically motivated, but then why should he, since he was concerned with foreign language teaching in the classroom? And even the directionality issue seems a dead duck when, as James (1980 9) points out, Weinreich (1953 88) observes that "the mother tongue . is in a privileged position to resist interference "

This is not to say that there are no directional effects, as studies of expatriate communities show (see e g Py, 1986) Also, in contradistinction to the predictions of the CAH, there are cases where there is an asymmetry in performance depending on which one of a pair of languages in contact is the L1 and which one the L2

For instance, Zobl (1980a) notes that French learners do not seem to produce utterances with preverbal object pronouns in English (je l aime - *I her like), while English learners of French frequently produce utterances like *le chien a mangé les Zobl believes that this is due to three factors 1) pronoun and noun behaviour is essentially NP behaviour in English In French this is not always so, there being a preverbal object clitic pronoun system, 2) although there is this system of clitic pronouns, they are avoided by children acquiring French as L1 Instead there is heavy reliance on deictic ça, and 3) historically, the preverbal clitics are the vestiges of an earlier preverbal pronoun system in French Since there appears to be a trend away from clitics in French, it seems the marked status of the weak pronouns is reflected in the behaviour of French learners of English The constraint on SOV ordering in the L2 holds good for other L2s which have weak pronouns, as Andersen shows for L2 Spanish (Andersen, 1983 192ff)

Dulay and Burt's point 3 above (That the majority of learners'

errors do not reflect the L1) is more difficult to deal with It is not clear whether Dulay and Burt are talking about error type or error token If the latter, then this statement is not very interesting. It is now well recognised that the L1 is by no means the only source of IL forms, as has been stated many times since Lado (see e g Wilkins, 1968) It would be more significant if it could be shown that most error types could not be related to the L1 of the learner This will, I contend, be much harder to do from the available evidence Let me illustrate the point with an example from the literature.

Arabski's (1979) analysis of the English of Polish learners in Poland is based on a corpus of 4263 errors His conclusion is that "more than half the errors are caused by transfer" (p 101) Strangely enough, Arabski does not count the 974 article errors in his corpus as being due to Polish, even though Polish has no articles as such, his reasoning being that there can be no L1 influence when there is nothing to do the influencing This is an overly restricted view of the role of the L1

Thus 23% of Arabski's corpus consist of article errors, which, if added to the errors Arabski does count as due to Polish, leaves us with a massive total of over 70% But of course an error count like this is misleading if used as fodder for arguments about the significance of CLI L1-like article errors are frequent because the obligatory contexts for article use are frequent in English, and the fact that Polish does not have an English-type article system may contribute to that high frequency As Larsen-Freeman (1978 377) has shown, articles are the most frequent of the morpheme tokens in both her L2 corpus and Brown's L1 corpus (Brown, 1973) (Finnish learners of English, as reported in Granfors and Palmberg, 1976, and Sajavaara, 1983, also omit articles freely - Finnish having no article system either - and make considerably more errors in this respect than their Swedish-speaking compatriots, who, having an article system, tend not to omit them See also Rosansky, 1976, Andersen, 1977, 1978, who claim a role for the L1 in the acquisition of articles)

This same insistence on quantification leads to statements which are surprisingly categoric According to Dulay et al (1982), only

8-23% of errors made by adult learners are attributable to the L1 (pp 102, 103, 109), and most of these errors are limited to word order violations. Thus the role of the L1, say Dulay and Burt, is minimal (For reasons why there should be any CLI at all in Dulay and Burt's account of second language acquisition, see pp 108-112 of Language Two.) Judging by p 183 of Language Two, the figures of 8-23% are based on information given in two small-scale studies (LoCoco, 1975, and White, 1977), both of which are highly limited in scope Yet Wode (1981 56) quotes the same two studies by LoCoco and White, inter alia, as counter evidence to Dulay and Burt's view of the limited role of the L1:

> It was found that Dulay's and Burt's view is too narrow, because L1 transfer does occur, beyond doubt, in many structural areas, as illustrated by those studies which were undertaken after the Dulay/Burt papers (for example, White, 1977, LoCoco, 1975, 1976 ...)

Part of the problem, as I suggest in Chapter Three, and as Wode also notes (1981:56), is that Dulay and Burt insist on an indelible association between CLI and behaviourism, such that any L1-like error that is not amenable to other interpretations becomes an embarrassment to their creative construction hypothesis

Recourse to quantification does assume some kind of (implicit) theoretical stance. To illustrate how different theoretical stances can lead to different claims, take Schumann's attribution of 50% of one of his subject's errors to L1 influence, on the basis of a linguistic comparison of adult Spanish and his subject's English (Schumann, 1981). Meisel (1980:29), operating from within a different perspective, is critical of Schumann's claim, since he argues against the pervasiveness of L1 influence, and, following in the wake of Dulay and Burt, seeks to show that mere linguistic identity of IL and L1 does not prove the existence of the process of transfer. Hence, in Meisel's view, Schumann overestimates the effects of L1 influence Here, both Meisel and Schumann are operating at a product level of analysis However, if one believes that L1 influence can be underdetermined by the product, then it is clear that Schumann's estimate of 50% could be too low

Like Meisel, whom they anticipate, Dulay and Burt adopt a

minimalist stance as far as a role for the L1 is concerned. There is a clear imperative to explain away L1-like errors as developmental, that is to say, as arising purely from the learner's attempts to grapple with L2 input independent of other linguistic sources, just as a child grapples with its L1 This gives rise, and rightly so, to classifications which recognise the etiological ambiguity of certain IL forms. A simple example of such ambiguity is preverbal negation in IL English in the case of learners whose L1s also have preverbal negation in mature syntax Researchers with different biases may indeed tend to maximalise the role of the L1 (what Faerch has called 'giving transfer a boost', Faerch, 1984). Dulay and Burt tend to minimalise the role of the L1 by only counting those errors that cannot equally well be ascribed to sources other than the L1 ('giving transfer a boot', we might say). The example I also quote below (Chapter Four) is *hers teeth*, produced by a Spanish learner. While at first blush this looks just like a calque from Spanish, *sus dentes*, Dulay and Burt suggest that it may come about as an IL generalisation of *hers* as an independent possessive pronoun, as in it's *hers*, to dependent modifier status. This explanation can be entertained, but the presence or absence of the clinching evidence, *hers tooth* or *mine teeth*, is conveniently not mentioned. Neither are errors which are common enough amongst Romance learners of English where attributive adjectives are pluralised (e.g. *all along the longs corridors*, where the above argument in favour of generalisation cannot be used)

If such errors as *hers teeth* can be explained away so simply, then it is little wonder that the residue of unambiguous L1-like errors is small And then again, Dulay and Burt tend to count tokens and not types; this leads to a situation where error tokens which do not reflect the L1 may predominate in any count. We have seen such a case in the Arabski study referred to above where nearly one quarter of the errors collected are due to articles alone. If we believe that such errors have nothing to do with the L1, then clearly the corpus of potentially L1-based errors is substantially reduced at a stroke. But all we have in fact done is to remove one or two types of error, or more correctly, domains of grammar where errors may occur. It is domains that should be counted just as much

as occurrences in a particular domain, if the need is felt to quantify error. Thus when Dulay and Burt (Dulay et al , 1982) tell us that less than 5% of the 513 errors made by the 179 child learners of English they studied (p 102) could be attributed to the L1, it is surprising to discover that the errors are made across only six syntactic structures (Dulay and Burt, 1974b, also Dulay et al , 1982 174ff) This means in effect an error rate of half an error per child per structure

In this study, Dulay and Burt choose six structures that contrast in the L1, Spanish, the L2 and English The errors are classified three ways, as Developmental (where they resemble English L1 acquisition errors), as Interference (where they reflect Spanish), and as Unique (where they resemble neither of these categories) When we turn to the six structures, we find that classification is somewhat simplistic What are scored are offences against a syntactic structure or part of a structure For structure 1 (NP-V-Pron), a Developmental error would be *The dog eat it* instead of *The dog ate it* An Interference error would be *(The) dog it ate*, based on Spanish *El perro lo comió* Since French children are said to avoid preclitics in their L1 in favour of deictic ça, (presumably to retain SVO ordering see Zobl (1980a) for discussion), perhaps it should not be surprising *mutatis mutandis* that *the dog it ate* is not found Furthermore, it is difficult to see how one could compare a tense error (*eat* for *ate*) with a word order error Structure 3 concerns negation, one of the most thoroughly studied areas of second language acquisition *He not eat* is presented as the prototypical Developmental error, and *Doesn t eat* (cf Spanish *no come*) as the Interference error Leaving aside the problem of how pro-drop errors are scored in this scheme, it is a mystery as to how this classification is arrived at *He not eat* could just as well be an Interference error It is very interesting to note that 22 out of the total of 24 reported Interference errors are made in this structure alone, and that 287 out of all 513 errors occur here Since *he not eat* could just as well be due to Interference, we must remove this structure from further consideration Once that is done, and we ignore structure 1 as well, we have reduced Dulay and Burt's type corpus to four

(without discussing them further), and the number of errors by over half. Furthermore we have reduced the number of Interference errors in their scheme to two, which amounts to about 1.5% of the remaining errors!

I want now to comment briefly on point d) above. This concerns Dulay and Burt's claim that positive transfer may not occur even when L1 and L2 are in some sense 'the same'. Dulay and Burt present some rather lightweight and unconvincing evidence in support of this claim, but I believe that this is a criticism of the original formulation of the CAH that is valid, if not quite in the way that they see it. Unlike them, I do not think that the lack of predicted positive carry-over into L2 means that the L1 must play no role in second language acquisition; it is rather that the role it plays is to constrain possible surface forms in the L2. It is this theme of constraints on the transferability of L1 forms that constitutes the main topic of the chapters below (Ch. 4 on idiomatic expressions, Ch. 5 on polysemous verbs, Ch. 6 on concrete metaphor, Ch. 7 on hypothetical conditionals).

If we now turn briefly to point e) above, we will see that Dulay and Burt insist that spontaneous oral 'communicative' performance provides the only interesting data for second language acquisition. Intuitional, metalinguistic, data are seen as inherently inferior (e.g. Dulay et al., 1982:103,183). Thus, the reasoning goes, any task which is not naturalistic cannot provide valid information about the role of the L1 in acquisition. This viewpoint is defensible as long as it is concerned with the collection of data amenable to these ideals of spontaneity and communicativeness. The trouble is that very little can be collected in this way beyond simple morphology, syntax, phonology, lexis and discourse. If one is interested in the acquisition of complex syntax or lexis, then it is necessary to be able to collect data in an efficient directed fashion, rather than rely on hit-and-miss methods. Thus there is an empirical necessity to devise experimental techniques which resolve this methodological impasse

Dulay and Burt reject out of hand:

Studies that suffer from serious methodological flaws (e.g the use of timed translation tests which encourage heavy reliance on the

first language) .. because their results cannot be reported with confidence (Dulay et al , 1982 103, fn 7) The reasoning for this rejection is not made explicit, but it must be assumed from the general tenor of the book that the problem lies with using data derived from unnatural experimental tasks Translation, and particularly translation under time constraints (though no references to such experiments are given) will encourage the use of the Monitor (with its store of low-level learned available-to-consciousness rules) and/or the L1 Thus such tasks do not provide a true picture of acquisition However, it has been argued (Harris and Sherwood, 1978) that translation skills are a natural concomitant of bilingualism, such skills being socially functional when used in the form of liaison interpretation between, say, family and government department Thus it may be argued that translation in itself is not a task restricted to the classroom or to experiments

While it is a general belief that translation tasks encourage use of the L1, the evidence is mostly anecdotal Learners do not inevitably resort to literal translation - on the contrary, there are data to show that they may fail to capitalise on correspondence between L1 and L2 (see e g Kellerman, 1979a,b,c and Chapter 8) Similarly, Liceras (1983) has suggested that translation as an elicitation instrument is a way of establishing learners' perceptions of *language distance*, that is how similar L1 and L2 are (see Chapter 3)

The evidence that Dulay et al present to demonstrate that translation stimulates overreliance on the L1 is, ironically, based on a study by Lado (1978 quoted in Dulay et al , 1982) Lado compared performance in a text translation task with performance on interpretation of the same text after a day's delay Subjects who translated made many more L1-based errors than subjects involved in the bilingual recall task

Again, it is difficult to evaluate these results A day's delay before interpretation is in effect a day's delay in recall It is well known that memory for surface forms decays rapidly, while meaning may persist in long term memory (Sachs, 1967) It is also known that memory span is greater in L1 than in L2 (Cook, 1979,

Meara, 1980) Ammerlaan (1983) found that with advanced university-level learners successive interpreting proved more accurate (i e free of linguistic error) from L1 to L2 than from L2 to L1, a finding which is contrary to normal expectation in simultaneous interpreting and translation Ammerlaan attributes this to the possession of a greater memory span in L1, permitting longer retention of more surface forms than would be the case with successive interpretations from L2 to L1 Memory for L2 surface forms might be expected to decay faster than in L1, and be unavailable for accurate recall for translation into L1 after a period of delay Underlying substance would of course remain (in so far as it was understood), but the learner would be free to determine the surface forms and avoid linguistic problems

A second point to be made about the possibility that elicitation procedures may encourage reliance on the L1 is that the question still arises as to what L1 items are most likely to influence the IL, and which L2 domains are most susceptible to L1 influence Years of research by many scholars have shown that not all the elements of the L1 are equally transferable Thus it is necessary to go beyond the observation that elicitation method or type of social interaction will activate more or less use of the L1 Until we can do that, we are in no position to establish a theory of L1 influence in second language performance

This critique of the most persistent attack on the notion of CLI has been conceived mainly to redress an imbalance in the second language literature, which till very recently at least, has tended to deny the L1 a significant role Despite their worthy insistence on examining processes rather than products, and their demonstration of commonalities in aspects of second language acquisition, Dulay and Burt spoil their case by cavalier treatment of facts and figures designed to serve their cause

The fact that the early behaviouristic approach to crosslinguistic influence is now generally viewed as invalid has not prevented the whole transfer question from re-emerging, albeit in new and more complex forms (see the contributions in Gass and Selinker, 1983 , for instance) One reason for this may be the fact that research into 'transfer-free' second language acquisition in

the seventies scarcely got beyond the grammatical morpheme and the acquisition of negation and WH-interrogation. This left very large areas of uncharted territory both within syntax and outside it where, in principle, L1 influence could play a significant role. Although the great goldrush days of Contrastive Analysis in second language acquisition may be over, the CLI issue has not turned out to be a salted mine. So much is clear from the existence of many purely taxonomic error analyses (e g Arabski, 1968, Duskova, 1969; Grauberg, 1971) and the continuing discussions of the value of the Contrastive Analysis Hypothesis for second language acquisition (e.g. James, 1971, 1980; Newmark and Reibel, 1973, Schachter, 1974; Taylor, 1975, etc., etc.) A preoccupation with the role of L1 influence permeates even the most recent development in second language acquisition research, as can be seen in current work within a Government and Binding framework (cf. Flynn, 1984, Mazurkewich, 1984; White, 1985).

1.3 Predicting the 'what' and 'when' of CLI

With the Ann Arbor conference in 1981 (Gass and Selinker, 1983), the study of crosslinguistic influence probably came of age. For one thing it was again recognised as a worthwhile topic in North America after a period in the doldrums. Secondly it revealed the breadth and scope of interest in the topic. And thirdly it showed how much more sophisticated methods of studying the phenomenon had become, and how much more sophisticated the theoretical underpinnings. Particularly interesting in this respect is the notion of constraint. Although there are some who would maintain that in language contact situations anything is transferable, as a rule in second language acquisition this is not systematically the case. We would want presumably to exclude the humorous exploitation of the 'ludic potential' (Sharwood Smith, 1982) of the L2 and thus the deliberate calquing so beloved of a certain sector of English society as seen in Miles Kington's weekly Punch column "Parlez-vous Franglais?".[5]

One of the principal goals of any theory of second language acquisition must be to account for the role of non-target languages. And not only their role in, shall we say, spontaneous informal

interaction outside the classroom, but also in the development of IL knowledge. As a first step towards the construction of that theory, we must aim to build a subtheory of transferability - what is the potential for transfer of a property of a given language to another language? Then, and only then, it is necessary to develop a theory of performance which makes allowances for CLI (cf. Sharwood Smith, 1985). This may well require an appeal to learners' knowledge in the form of acceptability judgements or other metalinguistic tasks, simply because there is no other way in which the relevant data can be gathered (see Chapter 6 below) Faerch (1984) criticises such a procedure when he claims that a serious limitation of the experimental approach using metalinguistic judgements is that.

> .. it is difficult to predict on the basis of such data alone how learners will transfer in specific communicative situations under various degrees of stress and with varying possibilities for consciously monitoring transfer (p 2)

However, such criticism is premature. We are simply in no position to make such predictions whatever methodology we use (Adjemian, 1983).

One researcher, Tarone (1982), has made explicit claims about how learners will perform in terms of what she calls a capability continuum. Tarone is interested in accounting for systematic variability in second language performance in response to different tasks, each task invoking the use of a different style depending on the degree of attention that has to be paid to form Tarone's claim is that when a learner's attention is diverted away from concentration on the form of utterances (as in grammar tests, for instance), the learner moves towards what she terms the 'vernacular', and is then the least likely to be influenced by his L1 The vernacular is the most stable and consistent style For Tarone, a learner's competence is therefore to be considered as being variable or heterogeneous To take one example, quoting Schmidt (1980), Tarone notes that the ellipsis of second verbs in conjoined sentences (e g. 'Mary is eating an apple and Sue a pear), never occurred in free oral production, 11% of the time in an elicited imitation task, 25% of the time in a sentence combining task, and 50% of the time in grammaticality judgements. Tarone

takes the absence of such ellipsis in the least form-oriented task as evidence that the vernacular (as represented by the free oral production task) is the least permeable to L1 and L2 variants. The vernacular is therefore least influenced by outside linguistic systems.

This interpretation is interesting, but suffers from a lack of clear empirical support, as Tarone herself admits. She examined performance on four morphemes, 3rd person singular -s, the noun plural morpheme -s, the article (which one is not specified), and the third person singular direct object pronoun (also not specified, but presumably him). The third person singular morpheme was indeed produced the most correctly in the most formal task, and the least correctly in an oral narrative. This is in accordance with the hypothesis. Performance on the noun plural morpheme did not vary according to task demand. However, behaviour on the remaining two morphemes went against the hypothesis, being least correct on the grammar test and most correct on the oral narrative. But as Rampton (in press) has demonstrated, the Labovian framework that Tarone espouses with its core concept of attention to form as a determiner of linguistic variability, cannot account for linguistic backsliding which is socially motivated; that is, the Punjabi child who uses *me no'* structures like *Me no take shower* to his teacher or his peers is not doing so because he is not concentrating on form, but because he is taking up a position of self-abasement, often, in the case of his peers, to downtone a boast, or so as not to appear contrary in front of his superiors. Thus *me no* structures are really expressing the notion of *It s only little me - don t take me seriously*, a form of defensive baby talk.

An alternative account to Tarone's would not propose a heterogeneous competence (or 'capability', as Tarone calls it), but a single one underlying all forms of performance at a given moment. Variable behaviour would be seen rather more in terms of the status of processing knowledge with which to access and retrieve that competence. Thus it is in the most careful tasks where the learner is not under pressure to perform under time and other constraints that we get the clearest reflection of his underlying linguistic knowledge. Maximum opportunity is thus afforded to processing

routines to access underlying knowledge This has been called psycholinguistic variability (Sharwood Smith and Kellerman, in press) as opposed to the sociolinguistic variability proposed by Tarone and others Thus production data can inform the researcher about the nature of that underlying knowledge, however, the learner not only possesses an interim grammar, but also ways of using that grammar with varying degrees of efficiency A particular change in the grammar has to lead to the development of mature processing mechanisms for using the new rule or feature Hence, performance data may also inform the researcher about the state of the learner's processing abilities with respect to some linguistic rule or feature To take a specific example, the appearance of an L1-like form in production may be interpreted as reflecting a hypothesised parallel between L1 and L2 on the part of the learner, i e a feature of interim competence, or it may be interpreted as a borrowing (cf Corder, 1983) from the L1 system If it is a borrowing, this may reflect the fact that a) there is a gap in the learner s L2 system, or b) there is no gap - the learner has a way of expressing a given meaning in L2 - but that there is insufficient automatisation, i e the appropriate processing mechanism is still immature and cannot function given the demand characteristics of the situation (See Sharwood Smith and Kellerman, in press, for further discussion of this point, and how it relates to the phenomenon of U-shaped behaviour discussed in Chapter 8) Thus Tarone's results can be seen as evidence of variable attentional demands on processing knowledge rather than variable competence In any case, Tarone s hypothesis is not concerned with specific claims about what may be transferred from L1 to L2, but only with the situations in which CLI will be enhanced or diminished There is no principled way in her framework in which predictions can be made about the transferability of L1 structures to the L2 Thus for Tarone's hypothesis to be of any interest to a theory of CLI, she would still have to be able to predict what sorts of structure were susceptible to transfer, and then make specific claims about her capability continuum with regard to particular structures

Tarone's work raises an important question - what do we use as the crucial data for a theory of CLI? If it can be shown that

different task demands lead to different types of performance, we must be extremely careful about the claims made on the basis of experiments It is particularly important to bear in mind the distinction between linguistic knowledge and processing knowledge Certain kinds of task may place great strain on processing mechanisms This point is specifically addressed in Ch 8 on U-shaped behaviour, where I attempt to reconcile apparently contradictory data sets It turns out that the data are contradictory only because the pragmatic contexts in which the data are elicited are quite different As long as this fact is kept in mind then the data can be properly interpreted within a theory that attempts to predict not specific instances of CLI but general probabilities, the particular preoccupation of Chs 5 and 6 If we can say that L1 property x is more transferable than y, then the onus of having to say that x is transferred while y is not is removed Instead, it must be shown that x is more likely to be transferred than y Furthermore, the notion of a psycholinguistic scale of transferability allows one to establish an independent metric which may be integrated into a theory of performance in second language acquisition

1 4 The empirical evidence for CLI

While work on CLI has proceeded apace, most of the emphasis has been on syntax Schachter and Rutherford (1979), Rutherford (1982 and 1984b) and Andersen (1984) have been particularly critical of this approach However, even in the supposedly universal morpheme acquisition orders, careful analysis reveals that the L1 does have a role to play Usually the basis for ascription of a particular phenomenon to the L1 will be formed by some sort of comparison between performances in a common L2 domain by learners with different L1s When differences in performance correspond to differences in language background, then it is reasonable to suppose, ceteris paribus, that the major reason for these differences is L1 influence Patterns of L2 behaviour which can be strictly limited to a subset of L1s (and this subset may contain a single member) provide the best resource material for an investigation of L1 influence

What follows is a brief survey of some of the recent work on CLI, showing the breadth of research into this topic. We begin with syntax, though the areas that fall under this classification cannot be viewed independently of discourse-pragmatic considerations that control their use Thus, the question of pro-drop is treated under this heading because there are pro-drop and non pro drop languages This is a morphosyntactic matter. The conditions governing subject deletion in pro-drop languages are of course discourse-bound; the same conditions do not generally lead to subject deletion in non-pro-drop languages.

1.4.1 Syntax

Of course, a great deal of work has been conducted in syntax, and much attention has been devoted to the role of the L1 in L2 syntactic development (Meisel, 1983, is a recent example) It has been fashionable to generalise from (morpho)syntactic studies to language acquisition in general in order to claim that CLI is at best a marginal phenomenon, but the generalisation is entirely unwarranted, as we shall see. In what follows, I shall deal with negation, word order, subject pronoun deletion, preposition stranding, dative alternation, and easy/eager to please.

a) Negation

Hyltenstam's (1977) is a well-known analysis of the acquisition of negation using implicational scaling, showing that learners with a wide variety of language backgrounds travel through the same stages with regard to the placement of the negative particle in Swedish in both main and subordinate clauses. A subsequent reanalysis by Hammarberg (1979) has suggested that learners with L1s not having pre-tensed verb negation, such as English, do not show evidence of the first stage where such negation is the norm. English provides a leg up, so to speak, along the developmental ladder, because as far as negation is concerned, English is more like Swedish than the rest of the source languages studied by Hyltenstam. Thus those learners whose own L1s have pre-tensed verb negation will have a harder time making progress than those whose L1s do not. This view is consistent with Schumann (below).[6]

Wode too (Wode, 1976) presents evidence that shows that the universal pattern of negation acquisition may be disrupted, albeit in restricted fashion, by the L1 under specific conditions, i e. where there is 'crucial similarity' between L1 and L2 He finds forms of negation in his Stages IIIa and b which also turn up in Ravem (1968), where Norwegian is L1, but are lacking in Milon (1974) - Japanese as L1, or Huang (1971) - Taiwanese as L1 Certainly, to take a Romance language, Spanish as an example, there is nothing like Wode's stage III in the data collected by Cancino, Rosansky and Schumann (1978). Thus a form like Marilyn like no sleeping, reminiscent of German post-tensed verb negation, appears to be a structure unique to learners with a Germanic background Schumann (e g. 1982) has suggested that while preverbal negation may be found in the basilang English of all acquirers, thus reducing the structural argument in favour of CLI, at the same time the fact that NO + V is the adult norm in certain L1s may delay the further acquisition of English negation [7] In fact, if the L1 is not a pre-[tensed verb] negating language, then this L2 stage may be a relatively fleeting one. Certainly this is the implication from Hyltenstam's study where it is the Serbo-Croat learners who take the longest to acquire Swedish negation syntax, and the English speakers who show virtually no sign of pre-[tensed verb] negation Interestingly, in Taeschner's (1983) study of the simultaneous acquisition of German and Italian by her two children, pre [tensed verb] negation structures persist in their German, while post-[tensed verb] negation is very rare in their Italian

b) Word order

As far as word-order errors are concerned, again there is some evidence for CLI, but equally well counter-evidence While it is clear that SVO order is carried over into German by learners with Romance L1s, Clahsen and Muysken (1983), in their summary of research into untutored acquisition in adults, argue for a basic SVO strategy in initial stages, even in those cases where the L1 is not an SVO language. Thus Turkish learners, though apparently transferring SVO order to German (Dittmar, 1981, quoted in Clahsen and Muysken), are in fact only superficially doing so, via a

complement preposing rule, which strands the verb in final position. Such forms as X-verb and X-subject-verb are also produced by learners with Romance L1s, where CLI cannot be an explanation, and where the most frequent word order is canonical SVO Of course the interesting case here will be to see if object preposing is employed significantly more by Turks than by Romance speakers in an effort to match Turkish word order. It is perfectly feasible that the SVO ordering of the initial stages of German acquisition is the result of transfer from L1 in some cases, i.e in the case of Romance speakers, or equally possible in the case of English speakers learning Dutch and German, where it may fossilise. Nevertheless, Clahsen and Muysken are right to emphasise that the presence of similar data from Turkish and Japanese learners of German does reduce the impact of the CLI argument and forces us to consider other possibilities On the other hand, it would also be necessary to take the case of the learner with an SOV L1 learning another SOV language. Would such a learner acquire the L2 order for free? In this vein, Rutherford (1983) reports that Japanese learners, like Turks learning German, do not use their L1 order, SOV, in English Clahsen and Muysken argue for an explanation of the preference for SVO order in terms of ease of processing via a 'neutral sentence type'. Needless to say, an SVO strategy will also meet with success in both German and Dutch However, in Jansen, Lalleman and Muysken (1981), the 'alternation hypothesis' proposed there suggests that when an L2 (like Dutch) offers two choices in word order, i.e SVO and SOV, in e g main and subordinate clauses, then if the L1 is an SVO language, the SVO pattern will be generalised, if the L1 is SOV, then it is the SOV pattern which will be generalised In main clauses it was found that Turks produced significantly more verbs in final position than Moroccans (Arabic is SVO) In subordinate clauses (where Dutch is SOV), Turks produce more verb-final structures than Moroccans, but the sample of such clauses is small There is some evidence in support of Zobl's claim that retardation may occur in the acquisition of the L2 when there is a developmental stage that resembles the adult L1 equivalent (Zobl, 1980c), in that the initial advantage that Moroccans have is to some extent cancelled out by their failure to break away from strict SVO

ordering, when there is a fronted constituent in initial position. Here Dutch would require subject-verb inversion, giving X-verb-subject, but Moroccans produce fewer of these latter structures than Turks - a subtle example of CLI Interestingly, in a study of Dutch foreigner talk addressed to non-Dutch speakers, Snow, van Eeden and Muysken (1981) found a large number of SVO orderings in subordinate clauses

c) Subject Pronoun Deletion and the Pro-Drop Parameter

Similarly, the role of CLI in subject pronoun deletion is not entirely clear. Though there is plenty of evidence that such deletion takes place in the L2 of Romance speakers (putative CLI), it has also been argued that such deletion is not restricted to those speakers whose L1s are pro-drop languages Meisel (1980) has shown that in the L2, Romance speakers drop subject pronouns more in the third person than in the first person, which fact cannot be explained by resorting to CLI (though it will be important to distinguish the grammatical implications of this observation from the pragmatic ones). And in case we should think that pronoun deletion is limited to speakers of pro-drop languages, Zobl (1984) reports some systematic deletion in French learners of English On the other hand, White (1985) found that while Spaniards accepted that pronoun deletion was possible in English, French learners did not Liceras (1983) studied performance in another aspect of the pro-drop parameter, namely that-trace violations immediately following a complementiser. That is to say, sentences of the kind *Who do you think that will come? are grammatical in Spanish, but not in English or French White notes that her Spanish subjects tended to accept sentences with that-trace violations in English, but then so did her French subjects Neither group tended to accept VS order in English, another attribute associated with the pro-drop parameter. Liceras reports that with increasing proficiency her English-speaking subjects stopped producing and accepting that-trace violations in L2 Spanish As with much of the current work based on Universal Grammar, White's results suffer from lack of methodological rigour (see section on Dative Alternation below), but what is particularly interesting about her work is the attempt to

reconcile Universal Grammar and CLI. In the case of a Spanish learner confronted with English, there is little support for the argument that the learner switches back to the unmarked setting of the pro-drop parameter (i.e. [+ pro-drop]; see Hyams, 1983), because if this were so, we would expect, following White's reasoning, to see acceptance of missing subjects in her French subjects. This does not occur. Therefore Spanish learners, who accept missing subjects in English, are still operating with the L1 parameter, even though English is a [- pro-drop] language. The difficulty with that-trace violations experienced by Spanish learners again suggests that the parameter is operative in English, but the argument is weakened by the fact that French subjects do not accept these structures in English either. Part of the problem no doubt resides in the fact that there are superficially similar sentences in English where the that is a relative pronoun (cf *Who do you think that will come? with Who do you know that will come?. Also the subtle meaning difference between Who do you know is coming? and Who do you know that is coming?).[8] Thus one wonders precisely why one has to invoke Universal Grammar or parameters to capture some rather unremarkable facts - learners with L1s where subjects may be dropped will drop them for a time in L2s where there is no subject deletion. As Foster (1984) reminds us, that they will drop them is a question of grammar. The conditions under which they will drop them is a question of pragmatics. The only other conclusion that can be reached for the moment is that it is hard to leave out complements before empty categories in the L2 if the L1 does not do this anyway (see also Hilles, 1985)

d) Preposition stranding

The arguments for CLI in the domain of English preposition stranding also seem unclear Mazurkewich (1983) showed that French learners move from more pied-piped sentences in English (e g 'To whom did John give the book?') to more preposition stranded ones (e.g. 'Who did John give the book to?'). French does not have preposition-stranding, and so this looks like a case of initial CLI, aided by the fact that pied-piped structures are usually grammatical in English [9] Although Mazurkewich claims that development is

apparent in Inuit learners of English (Inuit having no prepositions), that is to say there is a progression from unmarked pied-piping to marked stranding, and that therefore CLI cannot be an explanation alone, White (1984) observes that Inuit speakers use a higher percentage of preposition-stranded sentences irrespective of proficiency level when compared to the French subjects, and furthermore that the number of pied-piped constructions increases as proficiency grows, a fact that White attributes to the acquisition of more formal modes of English Liceras (1983), in her study of the acquisition of Spanish by speakers of English, finds that in judgement tasks, 43% of her subjects accepted preposition-stranding at the beginners' level, and also produced some in translation tasks (20%) However, in the case of the intermediate and advanced students, Liceras found that the marked preposition-stranding is to all intents and purposes not accepted or produced in the L2 It is difficult to draw conclusions from these findings since the L1/L2 pairings are different in each case It might perhaps be more useful to compare Mazurkewich s findings with her own findings on the acquisition of English dative alternation in Inuit learners, where it is also claimed that these learners proceed from the unmarked to the marked case (pied-piping before stranding, NP PP before NP NP) (see section e) below) In Liceras' case, the absence of stranding in Spanish suggests that the incidence of stranding amongst English learners of Spanish is pure transfer, and that markedness considerations are not relevant In the case of Mazurkewich's French subjects, the preponderance of pied piping is ambiguous from the point of view of etiology, since French does not allow stranding (with very minor exceptions), and thus pied-piping in the English of French learners could be interpreted as support for straightforward structural CLI and/or the theory that claims that the unmarked is acquired before the marked (Cf the similar sorts of argumentation in the preceding and following sections)

e) Dative alternation

The topic of dative alternation has recently received attention in two articles by Mazurkewich (1984, 1985) Since Kellerman (1985) is a specific reply to Mazurkewich (1984) but is not included in

this thesis, the following section will deal a little more fully with the claims of the latter paper Mazurkewich's basic claim is that a theory of markedness in Universal Grammar is also a theory of (L1 and L2) acquisition, such that *less marked* means 'liable to be acquired earlier' or 'easier to acquire', and *more marked* means 'liable to be acquired later' or 'harder to acquire' (cf the discussion of preposition stranding above) In terms of dative alternation, this would mean that unmarked [NP PP] structures (*John gave the menu to Cherry*) are acquired earlier in L2 English than marked [NP NP] structures (*John gave Cherry the menu*),[10] irrespective of the facts of the L1 (in this case French and Inuit) At first glance it does indeed seem as if Mazurkewich's hypothesis is supported by her data, since both French and Inuit subjects are more likely to accept the less marked [NP PP] sentences than the [NP NP] ones However, as also pointed out by White (forthc), Inuit speakers are generally accepting of *both* structure types (100% for [NP PP] and about 84% for [NP NP], and show no evidence of performing better as their nominal L2 proficiency grows Furthermore, Mazurkewich's English native speaker controls also perform 'better' on the former type, and as I show (Kellerman, 1985 98), there is in the main no statistically significant difference between Inuit and native speaker performance as to the judgement of possible [NP PP] and [NP NP] sentences On the other hand, most French learners *do* perform significantly differently from native speakers

If markedness is the key factor in the acquisition of dative alternation, then the question remains as to why performance between the French and Inuit learners is dissimilar Mazurkewich is not keen to invoke CLI, although by selecting learners with such disparate L1s, she certainly hoped to have been in a position to consider it (1984 99-100) For her, it is enough that both sets of learners are more likely to accept [NP PP] structures than [NP NP] ones However, it seems quite unreasonable to ignore the fact that in English French learners clearly prefer the only structure that is permitted by French, namely [NP PP] Furthermore, Mazurkewich's elicitation instructions to her subjects state that "there are no right or wrong answers" (1984 108) Since [NP PP] is extremely

common in English, is the only sanctioned form in French, *and*, as per experimental instructions, cannot be judged wrong, it is small wonder that the French learners prefer it to [NP NP]. We have here a case where the L1, L2 input and experimental design conspire to create a strong tendency in the results to support the markedness hypothesis. Inuktitut, on the other hand, has no prepositions; additionally, the Inuit were all "educated in English, although they received instruction in Inuktitut throughout their primary and secondary school years" (Mazurkewich, 1984 100). Despite attempts to match both experimental groups for proficiency, perhaps this fact alone explains why Inuktitut performance is so like that of native speakers, at least in this grammatical domain.

f) Easy/eager to please

Bongaerts (1983), in a partial replication of L2 experiments based on the comprehension of complex structures in L1 acquisition, found that his Dutch subjects had far fewer problems with English *easy to see*-type sentences than learners with French, Arabic and Hebrew backgrounds. This is most likely not only due to the fact that *easy to see* structures are commonplace in Dutch (but not in the other languages studied), but also that Dutch speakers are used to making the semantic distinction between *easy* and *eager* sentences without the benefit of explicit syntactic marking, thus demonstrating that, although Bongaerts' subjects did make some errors in comprehension of these structures, the fact that they had prior experience of them via their L1s gave them a decided advantage over learners whose L1s did not have them.

1.4.2 Discourse phenomena

Let us now turn to some cases where CLI is not immediately apparent because it does not manifest itself in a surface structure echo of the L1 that can immediately be detected and ascribed to its source. A spectacular example is in Schachter and Rutherford (1979), who showed that what appears to be superficially a malformed passive in English produced by Mandarin and Japanese speakers tended on close analysis to be nothing of the sort Instead, it is argued,

such structures as *Most of food which is served in such restaurant have cooked already* or *If I have finished these four jobs, I am confident that my company can list in the biggest 100 companies in the world* (Schachter and Rutherford, 1979 7-8) are actually attempts to transfer the topic-comment function of Chinese and Japanese to English form Rutherford (1983) points out that the difficulty in recognising this sort of transfer comes about through failure of researchers and teachers to look beyond syntax *qua* syntax and consider surface forms in terms of their function in discourse

A similar phenomenon to the above exists in the English of advanced Dutch learners A sentence such as *In the so-called R.E.M. phase dreams occur* is usually adjudged by native speakers to be at least 'uncomfortable' and typical of the Dutchman's tendency to pile up adverbial phrases in front of the verb and subject (inverted in Dutch, but rarely in Dutch-English) Although native speakers of English cannot usually say why such sentences are odd, it is because they violate the principle of 'end-weight' (Quirk et al , 1972) Thus the usual correction offered is *Dreams occur in the so-called R.E.M. phase*, but the trick is to put this sentence back into its original context in discourse

There then follows a period of rapid eye movements
In the so-called R.E.M. phase dreams occur

The native speaker correction with postposed prepositional phrase does not fit well with the requirements of discourse continuity here, and the appropriate correction would be in the form of a cleft sentence, viz

There then follows a period of rapid eye movements
It is in the/this so-called R.E.M. phase that dreams occur

In fact, our learner makes his error by following the discourse requirements of given-new, correctly applies local intrasentential syntactic rules (e g no inversion after a preposed element), but in so doing violates the principle of end-weight only in those cases where sentence structure is XSV This is a fairly subtle form of CLI, but one which is not infrequent in the written discourse of

Dutch writers of English

Another study which shows that errors which appear to be engendered by confusion between grammatical function and word order within the sentence are in fact to be interpreted within a discourse framework is Jordens (1983a,b, 1986) who examines the case-marking errors made by advanced Dutch and English learners of German (neither L1 being a case-marking language) Such errors occur when objects are incorrectly marked as nominatives, and subjects as accusatives, viz.

*Jeder Republikaner betrachtete er als sein
personlicher Feind
(Jeden. seinen personlichen)
"Every Republican he considered a personal enemy of his"

*Dadurch entsteht einen Knall (ein Knall)
(Through that comes into existence a bang)
"That causes a bang"

Jordens argues that these errors are not caused simply by the failure to recognise the syntactic functions of subject and object, the sentence-initial NP being marked as nominative, and the sentence-final one as accusative Rather these errors arise from L1-based semantic intuitions about the functions of the NPs within the overall discourse framework Thus grammatical subjects are given accusative case-marking when they are the focus (and particularly when they are semantically patient), and grammatical objects will be marked as nominatives when they constitute the topic.

Another researcher to investigate CLI in discourse is Trévise (1986), who looks at the range of topicalisation devices available to and used by English learners of French, and French learners of English. Trévise shows that French learners do not transfer 'non-syntactic' or pragmatic forms of topicalisation, but stick to more neutral syntactic devices permissible in 'le bon usage', such as 'canonical' SVO (not so canonical in spoken French) and extrapositional and identificational constructions [*It s really great New York; Portugal it s very difficult to live there*] Since Trévise's subjects were all learners of English in formal settings, it is quite possible that their unwillingness to transfer most of the possible topicalisation devices of spoken French comes from a

norm-oriented desire to avoid the worst excesses of colloquial speech. Even quite unexceptionable examples of thematic fronting found in English and French were never produced, so we cannot rule out the influence of the institutional setting. In the L2 French of English learners, SVO structures and anaphoric c'est were frequently used (*Le groupe ici c est très divisé*) Trévise's conclusion is that learners tend to choose a few neutral topicalisation devices, which generally do not lead to error (although they may not always be felicitous). Like the other studies mentioned here, Trévise's findings seem quite unambiguous.

Von Stutterheim's (1982) studies of the German discourse of Turkish guest workers reveal patterns that do not appear in other types of L2 German For instance, in the expression of temporality, Turks attempt to maintain in their German the two past tenses of Turkish. The remote past tense in Turkish is not relational (as the pluperfect in German or English would be), but strictly deictic. Before Turkish speakers develop tense marking in German, they express the distinction between remote and recent past via the use of gradable adverbs like *ganz früher* - "totally earlier", and thus remote, vs. *früher* - "earlier", and thus recent *Ganz vorher* - "totally previously" and *vorher* - "previously" are similarly used More advanced speakers will use the German pluperfect for the remote past. Von Stutterheim also notes that Turkish speakers produce *sein* ("to be") plus infinitive structures to maintain the imperfective/durative vs. perfective/punctive distinction of their native language. Forms like *Ich bin lesen/gehen* - "I am read/go" - are, however, only used with verbs that are not inherently durative. Structures like *Ich bin bleiben* - "I am stay" are thus not attested. There is also a habitual use of this uniquely Turkish structure in L2 German, as in *Ich bin immer busfahren* - "I used to be a bus driver". These durative structures are superficially akin to progressive structures in English (there is no equivalent in German), but in English, duration does not have to be marked by the progressive (*they relaxed in the hot tub every Monday; he read the book over several years*); interestingly enough, advanced Dutch learners use the English progressive duratively (and incorrectly), as in *Day after day he was swotting for his exam* or *It was raining*

for hours

1.4.3 Lexis and semantics

a) Lexis

There are enormous quantities of evidence for the influence of the L1 on IL when it comes to lexis, yet such is our obsession with syntax, morphology and discourse that lexis tends to get overlooked. One language context which has provided an opportunity for close examination of how learners with typologically remote L1s learn a common L2 is that prevailing in Åbo, in the south of Finland, where there is a Swedish-speaking enclave with its own university. Despite the existence of the two languages, their speakers live together harmoniously in one culture, though as one might expect, while Finnish is a second language for the Swedish-speaking Finns, for the Finns, Swedish is more of a foreign language A long-term project has been under way for a number of years in which the English of Finnish- and Swedish-speaking Finns has been systematically analysed with a view to determining the differences in proficiency between the two groups. This situation is an ideal one for studying the effects of the L1 on the learning of the L2, especially since Swedish is so clearly related to English and Finnish is not A recent paper by Sjoholm (1983) will serve as illustration. He studied 723 learners of English (309 with L1 Finnish ['Finns'] and 414 with L1 Swedish ['Swedes']) at three different levels of proficiency. Sjoholm's first test concerns prepositions, where Finns make many more mistakes than Swedes, especially where the former group rate their responses to be completely correct (c. 56% of responses for Finns, and c. 36% for Swedes) It is worth noting that Swedish has prepositions and Finnish does not The Finns use considerably fewer (15%-20% fewer) Swedish-based solutions, and, it is reported, 'did not have any great appetite for Finnish-based solutions, whereas Swedes clearly avoided them ..' (182) So much, says Sjoholm, is true for prepositions, but it is not true necessarily for other elements in the L2, such as idiomatic expressions, where both language groups tend to reject English idioms apparently modelled on their own L1s whether actually permissible or not. (Cf. the findings in Chapter 4.)

Ringbom (1978) examined the lexical errors made by Swedish and Finnish Finns, and found interesting patterns. Loan translations or extension of the semantic range of lexical items in English could be very clearly related to the L1 - the majority of errors could be attributed to the meanings of partial translation equivalents being transferred. Only two errors made by Finns could be attributed to a Swedish meaning being incorrectly transferred, while there were 14 attributable to Finnish amongst the Swedes When it comes to formal morphological similarity, Swedish is the overwhelming source of error for both sets of learners Language switch is uniquely from Swedish, and word blends of L1 and L2 are almost always Swedish-based Anglicised nonce words are commonly based on Swedish and rarely on Finnish And interlingual confusions also follow the predictable pattern. Thus on the formal level only, of the 187 errors collected by Ringbom from Finns, 162 are attributable to Swedish (c. 87%), for Swedes, the figures are 315 out of 322 (c 98%). Wikberg (1979) also reports that L3 interference (i.e. from Swedish) tends to be found more among Finns than among Swedes, but that Finns are more inventive in word-building, Finnish being very rich in derivational processes

b) Semantics

Some interesting directions are being pursued by a number of researchers interested in the way that CLI works when L1 and L2 lexicalise semantic categories differently. We have already seen that a vast number of the errors reported by the Åbo team could be related to the attachment of meanings to English translation equivalents appropriate only to Finnish or Swedish, and the English is accordingly underdifferentiated (see also Arabski, 1979). In such cases, it seems as if learner behaviour can be 'explained' on the basis of superficial comparisons of the L1 and L2. However, some bodies of learner data cannot be accounted for in this way A number of studies seek to explain these data by appealing to the notion of semantic 'core' or 'prototype', e g Gass and Ard (1984), K Flynn (1983), Hoeks (1985), Ijaz (1985) and Kellerman (e g 1978, 1980, and Chapters 5 and 6)

Gass and Ard discuss the notion of 'core' meaning for certain

English tense/aspect combinations such as the present progressive ('ongoing, witnessed activity which persists for an extended period of time' - Gass and Ard, 1984, where they note that an event in which current continuous activity is highlighted linguistically is closer to the core in English than an event which lacks this feature. Degrees of 'coreness' for tense/aspect systems are established independently such that

I am driving a Fiat now

is more 'core-like' than

I am flying to L.A. tomorrow

(see discussion and references in Gass and Ard, 1984). Gass and Ard (1984) examine the L2 judgements of a large number of Japanese and Spanish learners as to the correctness of English sentences using the present progressive (and other tenses) either in isolation or contextualised In the case of the progressive, subjects were more likely to judge sentences as correct the closer the interpretation was to the core, (i.e *John is smoking American cigarettes now*). The less core-like the meaning (i e *John is travelling to New York tomorrow*), the less likely the sentence was to be accepted In writing tasks, where learners were required to write sentences of their own choice based on uncontextualised sentences, there was a strong tendency to select only core-like meanings Similar results are reported for the simple present and will-future Thus I smoke cigars is seen as more acceptable than *Pan-Am flies to London tomorrow*, and *I will go to the Library tomorrow* is, as one might have expected, more likely to be accepted than **I will do it last night* While there is a general similarity in performance between the two groups, there are some differences For instance, Spanish learners perform differently from Japanese learners on the following progressive sentence pair

Dan is seeing better now

John is travelling to New York tomorrow

Spaniards overwhelmingly judge the first as more acceptable than

the second, while the Japanese tend to reject both (the first more heavily than the second). Since Gass and Ard claim that both sentences can be literally translated into Spanish,[11] they hypothesise that because of the greater similarity between Spanish and English verb semantics, judgements about English will be affected by intuitions about Spanish, and consequently the futurate use of the progressive, seen as non-focal in Spanish, is rejected in English While we are not told what the facts of Japanese are, one assumes that they are very different from English Consequently, and consonant with Kellerman's findings for the semantics of *break* (Chapter 5), Japanese learners do not use the L1 as a source of predictions about English, and tend to accept only the focal meaning Thus there are no CLI effects from Japanese to English, while they do seem to be apparent from Spanish A possible difficulty for this research (apart from acceptability questions in Spanish) is that learners' judgements could be heavily biassed by pedagogical strategies One would expect the core meanings to be the first to be taught, and so these results may equally well reflect teaching input Gass and Ard's findings are compatible with those discussed in Chapters 5 and 6.

K. Flynn (1983), in a similar experiment, looked at the use of the present perfect in the written work of Chinese, Arabic and Spanish learners of English She noted that although there were no significant differences in frequency of perfect use between the language groups, there were differences in use of the various functions of the perfect Using an analysis of the perfect given by Comrie (1976), who divides up the perfect into a) *result/state perfect* - Technology has produced changes in my country, b) *the experiential perfect* - I have never seen a woman president in my country, c) *the perfect of persistent situation* - Women have been working in those fields since the revolution in my country, and d) *the perfect of recent past* - Recently women in my country have chosen to work out of the house (all examples from Flynn, 1983), Flynn shows that Spaniards used significantly more experiential perfects and considerably fewer recent past perfects than other groups, while Arabs used significantly fewer persistent situation perfects. These results are extremely interesting in that they

again point to language-specific differences that only become apparent in comparative L2 data One major problem with this kind of research, to some extent obviated in Gass and Ard's more controlled framework, is that by using free production, Flynn leaves herself the arduous task of categorising the L2 perfects, no easy matter when it comes to subtle distinctions of meaning contained within a single form (Herman Wekker, personal communication)

Hoeks (1985), using similarity ratings for common Dutch nouns each with at least two meanings (e g loods = "river pilot, warehouse", roos = "rose, bull's eye, dandruff") found that the greater the similarity between the meanings as perceived by native speakers, the more likely a single English translation equivalent would be selected by Dutch learners When the meanings of the L1 noun were not seen as similar enough, predictably two different nouns were chosen in English The error patterns reveal that there is clear evidence of the transfer of semantic distance to English Which of the two meanings of the Dutch noun would be allocated to which English lexical item was very largely predictable from a free production test in Dutch in which subjects were asked to provide an instant sentence illustrating the meaning of each noun Thus, to take one example, the majority of subjects provided a sentence containing a 'warehouse' interpretation of loods, which meaning was then considered to be the prototypical one This preference for the inanimate meaning may reflect 'felt preponderance', 'warehouse' being less situationally bound than 'river pilot' Objective frequency counts do not help here as they do not distinguish the frequencies of the meanings of ambiguous words The less dominant sense was more likely to be translated incorrectly or not translated at all

Ijaz (1985) looked at English prepositions (e g on and over) as used by native speakers and learners Using multidimensional scaling of similarities between putative prototypical meanings of the prepositions as rated by various groups of subjects, and data from a sentence completion task, Ijaz found evidence for what she calls the semantic equivalence hypothesis This hypothesis claims that learners will assume that formal translation equivalents are also functional equivalents, i e share the same set of meanings

However, while it was true that all learners transferred the more prototypical meanings of translation equivalents quite happily, more advanced learners perceived non-prototypical senses of L1 prepositions as non-transferable. In such cases, learners failed to capitalise on correspondences, instead producing errors. Consequently, less advanced learners performed better because they were willing to assume full correspondence between L1 and L2.

These results are fully consistent with those reported by Kellerman in various places (see particularly Kellerman, 1979a, 1983; Sharwood Smith and Kellerman, in press, and Chapter Eight for a discussion of the implications of 'recidivistic' performance) [12] What they suggest is that a critical level of proficiency has to be reached before learners abandon what appears to be a relexification strategy. As Mogensen (1984, quoted in Faerch and Kasper, forthc.) points out, the availability of alternative solutions to transfer will no doubt aid the learner in moving away from such a strategy; thus it would be unwise to assume that 'relexifiers' necessarily know no better. It may just be that they can do no better. Consequently, there may be good reasons for favouring judgement tests over translation tests. The former make different (and less arduous) demands on less proficient learners and do not force them to seek linguistic solutions with often severely limited L2 resources.

One recent study investigating lexicalisation of the notions of *movement*, *manner* and *direction* in verbs is Harley (in press), who looked at the written French compositions of Anglophone students enrolled in an immersion programme and of native French-Canadians. She noted that the immersion students relied more heavily on prepositions than the native speakers, as they would in their L1, to express direction as a separate lexicalisation outside the verb. English, as Weeks (1983), following Talmy (1975) shows, tends to conflate movement and manner within the verb (*She DANCED into the room*), while Romance languages lexicalise movement and direction within the verb (*Ella ENTRÓ al cuarto BAILANDO* - She entered the room dancing) In French, unlike in English, the most common prepositions are neutral in terms of direction versus location (with exceptions, of course); in English direction and location are

usually indicated by different prepositions (again with exceptions). To sum up Harley's results, immersion students tended to use more non-directional movement verbs and prepositions than native speakers, who, if they used such verbs, always did so with an explicitly directional preposition or prepositional phrase. Thus to take the relative uses of courir (to run), a verb which conflates movement and manner, but not direction - a typically English pattern - the immersion students used this verb far more frequently than the native speakers, and where direction was involved, in conjunction with a preposition(al phrase) like à unmarked for direction (le chat courait à la maison). Thus for native speakers, most prepositions drew their directional interpretation from the verb, while for immersion students, direction was frequently carried by the preposition alone. As Harley herself notes (236), we certainly need to know what Francophone learners of English will do in similar circumstances. We do know from Bowerman's research into L1 development (1982a) that children make conflation errors in English, producing examples like *I m frowning out the door* and *We crouched down the hill* (examples quoted in Weeks, 1983), but among second language learners one might expect to see the Romance pattern predominate (even though it is contrary to the general Indo-European trend) because it allows the expression of movement and direction in a single lexicalisation, while manner, often a subsidiary requirement in communication, is lexicalised optionally.

Another study making use of comparative quantitative data is Wong (1983). She investigates the relatively greater number of *make* constructions in the written compositions of Chinese learners of English than in the work of other learners She claims that the predilection for *make* structures is directly relatable to Chinese, where the relatively few lexicalised causative verbs are literary or technical, and the most frequent structure contains an explicit marker of causation (shi, make) + complement. Wong's claim is that the absence of lexicalisation in Chinese will lead to under-lexicalisation in English with resultant unidiomatic usage, such as *They might make their friends get very upset about this*, instead of, presumably, *They might upset their friends very much about this*, where "to upset" in English is a lexicalised causative

Given that analytical *make* + complement is well-known in studies of L2 as a means of circumventing lexical holes we see here a case where the structure of the L1 encourages a tendency towards grammaticalisation already present in interlanguages in general which is probably of a greater order than in either L1 or L2 (see Haiman, 1980, for the relationship between lexical roots and grammatical processes in language)

1 5 The art in a state CLI and the case of the pronominal reflex

In recent years one of the most thoroughly researched areas of L2 grammar has been the relative clause Nearly all of this research has been based on the work of Keenan and Comrie (1977) on the Noun Phrase Accessibility Hierarchy While there seems to be a consensus that the hierarchy does indeed predict the course of learning types of restrictive relative clauses in terms of their relation to the head noun, such a measure of agreement is perhaps premature in view of the different methodologies and results obtained The appropriate methodology to use is a problem that plagues current research in second language grammar (see Long and Sato, 1984, for a thorough review) Perhaps because the field is a young one, there seems to be a tendency in some quarters to present weighty theoretical apparatus and skimp on the experimentation As often as not, the results of the experimentation, thin though it is, support the weighty theoretical apparatus, and the correlation between theory and empirical evidence is taken as read Here we have a perfect example of the tyranny of print Recent papers espousing a Government and Binding/Universal Grammar framework are particularly susceptible to this criticism (S Flynn, e g 1984, is an exception to these strictures) The use of grammaticality judgements requiring yes/no responses seems to be the standard method of investigation, but without the necessary controls If we accept that grammaticality judgements are complex behavioural acts (Carroll, Bever and Pollack, 1981) in their own right and do not necessarily provide privileged access to underlying competence, since such judgements are manipulable by variable presentation conditions, then it is clear that great care must be taken in the use and interpretation of such judgements Sorace (e g 1985 and

ms) argues most forcefully that the unstable nature of learners' grammars requires particular watchfulness on the part of researchers Sorace points out that relatively little attention has been paid to the reliability and validity of the elicitation instrument, and measures of learners' consistency and certainty of judgement have rarely been incorporated into the analyses of results (Kellerman, 1974 also uses a certainty measure to demonstrate that learners do not produce only the grammatical structures they are certain they know - see below)

One of the first papers to examine the acquisition of relative clauses by second language learners is Schachter (1974) This paper is now something of a classic because of its discussion of *avoidance* Schachter made use of then recent work by Keenan and Comrie (1972) on relative clauses and the Noun Phrase Accessibility Hierarchy, which showed that in the world's languages there was a general ordering of relative clause types according to the function of the relativised noun phrase head in the clause To cut a long and familiar story short the Accessibility Hierarchy is Subject > Direct Object > Indirect Object > Object of Preposition > Possessive > Object of Comparative Particle If a language can form a relative clause of a given function, then it will also be able to form relative clauses higher up the hierarchy, but not necessarily lower down Also if a language requires a pronominal reflex of the NP head in a given relative clause function, it will also require a pronominal reflex in every type of relative clause it can form lower down the hierarchy, but not necessarily higher up Schachter analyses the written performance of 50 learners of English with four different language backgrounds (and a native control group) and finds that Persians and Arabs produce as many English relative clauses as native Americans do, but between two and three times as many as Chinese and Japanese learners In doing so, the Persians and the Arabs make the most errors (in 20-25% of all relative clauses), but the Persians make many more errors due to illegal insertion of a pronominal reflex than Arabs (81% vs 39% of all errors) Schachter's point is that orthodox techniques of error analysis cannot deal with data of these kinds, since it is likely that Chinese and Japanese learners are

producing considerably fewer relative clauses than Persians and Arabs because they are left-branching in the L1 rather than right-branching as in the L2. Thus Chinese and Japanese learners only risk relative clauses in English when they are sure of getting them right. Schachter does not tell us how many of the very few errors which Chinese and Japanese learners do make are caused by the faulty insertion of a pronoun reflex, but such figures would be meaninglessly small anyway. Secondly, when it comes to errors of pronoun reflex insertion, Keenan and Comrie's data suggest that obligatory pronoun reflex insertion is maximal in Persian relative clause formation (with the possible exceptions of Subject and Direct Object). Hence the higher proportion of pronoun reflex errors amongst Persians is to be expected, if one accepts Schachter's account of the facts of pronoun reflex insertion for the various L1s in her study She makes use of Keenan and Comrie, as I have already indicated, but Keenan and Comrie (1972), which is crucially different from Keenan and Comrie (1977) What is more, according to the 1977 version, the Chinese should have more problems with pronoun reflex retention than the Persians. That this is not evident can be accounted for by the much stronger force of avoidance of difficulty in relative clause formation. Consequently, on the basis of the cross-linguistic data presented in Keenan and Comrie (1977), it is not at all clear that there is L1 influence in pronoun reflex insertion, while relative clause position may be an area of difficulty for Chinese and Japanese learners which is not experienced by Persians and Arabs Eckman (1977) is critical of Schachter in the sense that although her notion of avoidance is intuitively plausible, it should also be demonstrable in other ways, i.e. by the commission of forced errors (see also Kleinmann, 1978). Eckman's own hypothesis, based on notions of markedness in universal grammar and Keenan and Comrie (1972), offers different CLI predictions from Schachter based on error production rather than overall relative clause frequencies. However, as Kellerman (1979a) points out, Eckman's predictions depend critically on the relative clause data provided by Keenan and Comrie (1972). They are not supported by the data given in Keenan and Comrie (1977) nor, for that matter, in Gass (1980) Tarallo and Myhill (1983) provide

different data again, claiming that pronoun reflex is restricted to Possessive in Chinese. Yet according to Comrie (personal communication), for Peking Mandarin pronoun reflex is sometimes required on Direct Object and obligatory in all other positions except Subject.

Schachter's follow-up work on relative clauses is more problematic (Schachter, Tyson and Diffley, 1976). As far as pronoun reflex insertion is concerned, Persian and Arab subjects were asked to judge the grammaticality of sentences with grammatical English relative clauses and 'English' relative clauses built on the L1 model with pronoun reflexes. Persian speakers accepted 87.5% of the former category and 80% of the latter, while Arabs accepted 82.4% of the former, and, if they were not also Francophone bilinguals, 80% of the latter (Jordens, 1977). (The inclusion of the bilinguals reduces this latter figure to 61%.) Chinese and Japanese learners also accepted English relative clauses (74.2% in both cases), but operated at around chance for sentences based on their own L1s (60% and 54.5% respectively).

There also appeared to be a tendency to behave randomly towards relative clauses based on the relative clause patterns of other L1 groups, though the Japanese group accepted these to the tune of 68%. This set of results is difficult to interpret, since it suggests that learners may entertain more than one hypothesis about relative clause formation, at least passively. It is true on the one hand that all groups more or less know a sentence with a correctly applied relative clause when they see it, but it is also true that they act variably towards those modelled on their own patterns. There is some support for Schachter (1974), in that it is the Arabs and the Persians who are the most accepting in L1-like variants. This suggests that their production figures as noted in Schachter (1974) are due to a failure to perceive the differences between L1 and L2. However, an explanation is still required for the fact that Arab bilinguals do not accept Arabic-based sentences, as it is for the non-random behaviour of the Japanese group to sentences modelled on other L1s. (See also Sorace ms..)

Consequently one cannot be too confident about the validity of a study by Ioup and Kruse (1977). Ioup and Kruse set out to determine

whether CLI or inherent structural complexity (structural complexity corresponding to position on the Accessibility Hierarchy, with Subject the least complex) was the conditioning factor in L2 performance in relative clauses. They too are critical of Schachter (1974) for postulating avoidance on the basis of production data, but they are right to point out that studying free performance alone can lead to the underdetermination of the learner's competence. Consequently they also used intuitional judgements as their data base with "low" and "middle" *intermediate* learners from five different language backgrounds (the four used by Schachter, plus Spanish, which reputedly has no pronoun reflex). As far as pronoun reflex retention is concerned, Ioup and Kruse state that all groups seemed to have pronoun reflex problems (even though they are only concerned with Subject and Direct Object relative clauses), and that includes the Spanish group, who produced some 47% incorrect judgements (as against the Arabs' 68.5% at the one extreme, and the Japanese' 35% on the other). The mean error rate for pronoun reflexes is higher than for any other category, a fact which clearly puzzles the authors. However, it is not at all surprising that pronoun reflex sentences should not prove to be good discriminators by native language, given the elicitation format. It is unreasonable to expect that just because a particular structure (in this case a pronoun reflex) is embedded in a relative clause, and then in a sentence, a learner will automatically be judging the sentence in terms of that structure let alone the presence or absence of a pronoun reflex. For instance, Ioup and Kruse offer *The girl who she chased the bird fell down* as one of their malformed pronoun reflex sentences to be judged. If this is to be judged for acceptability of pronoun reflex, then the first requirement is that *girl* and *she* should be seen as co-referential. Of 15 advanced Dutch learners of English I recently asked to judge, and if necessary, correct this sentence, 8 reformulated it deleting the pronoun reflex. The remaining 7 each gave a different response suggesting that their problems lay elsewhere than with the pronoun reflex.

With some of the problems of previous research in mind, Gass (1979, 1980; Gass and Ard, 1980, 1984) conducted a series of experiments on relative clause performance. Gass used three tasks,

a grammatical judgement task, a sentence-combining task and a free composition task. Gass shows that in the free composition task, the percentage of relative clauses used follows Keenan and Comrie's Accessibility Hierarchy. That is to say, the further down the hierarchy one goes the less frequent the relative clauses become. The sentence combining task also yielded results which tallied well with the Accessibility Hierarchy (with one important exception, Possessive, where learners performed better than with all other relative clause functions except Subject) In the judgement task, Gass noted a possible transfer effect in that subjects with [+pronoun reflex] L1s were more likely to accept sentences with pronoun reflexes in them in the first three positions of the hierarchy than subjects with [-pronoun reflex] L1s. By using both judgement and combination tasks, Gass increases the validity of her findings, compensating to some degree for the very low number of subjects in her experiments (N=17).

Turning now to another study, Hyltenstam (1983) set out to examine the role of different elicitation instruments in L2 data production. He compares written compositions, picture identification (oral), imitation, and an oral and a written judgement task. Like Gass, Hyltenstam had (small numbers of) subjects divided according to whether their L1s were [+pronoun reflex] or [-pronoun reflex] Hyltenstam's subjects had been in Sweden for two years or less, but were considered to be 'at a fairly advanced level'. The results are in some senses like Gass' In the free composition task, the vast majority of relative clauses are Subject (c. 88%). There were only 9 pronoun reflexes Similarly a native speaker control group also only produced Subject and Object relative clauses. In the picture identification task, regular implicational patterning was apparent in individuals in accordance with the Accessibility Hierarchy Interestingly, all four groups showed pronoun reflex insertion, though none in Subject position - the Persians in 64% of cases, the Greeks in 54% (the two [+pronoun reflex] languages), the Spaniards in 39%, and the Finns in 12 5% (the two [-pronoun reflex] languages) In the judgement tasks, on the other hand, the results are very messy indeed, and no scaling is possible. Looking quickly at the data, all one can say is that some

subjects tend to accept pronoun reflexes at least some of the time in all positions, the [+pronoun reflex] group somewhat more than the [-pronoun reflex] group Even grouping the scores does not create order out of the chaos of individual variation. The imitation session produced little of interest, with only 11 pronoun reflexes counted. Hyltenstam's inevitable conclusion is that different elicitation tasks are not always equally suitable in gathering data. Only the oral production task produced patterned results and pronoun reflexes in interesting numbers. Hyltenstam also checked all his techniques with native speakers and reported extremely stable results Once again, these results call into question the validity of other findings.

In a pilot study, Liceras (1983) found the sentence-combining task used by Gass unsatisfactory since the majority of sentences created produced Subject relative clauses or no relative clauses at all. Like Gass and Hyltenstam, Liceras notes that the number of Subject relative clauses produced in free compositions is very high relative to other relative clause types This was true for both native and non-native texts. Ultimately Liceras uses translation, grammatical judgements and a fill-in-the blank task as her elicitation instruments. Liceras had four groups of subjects, 50 in all, of which 45 were Anglophone learners of Spanish at three levels of proficiency (beginners, intermediate and advanced), and 5 native speakers of Spanish.

Liceras' findings are as follows: pronoun reflexes are generally accepted by the beginners in the judgement task, and substantially so in the intermediate group. Her results do not confirm the hypothesis "that resumptive pronouns would be more accepted in the 'most' difficult positions" (194), and they were accepted in Subject position. These results derive from grouped data. Advanced learners and the native speakers showed very little tendency to accept pronoun reflexes. One finding is that if the NP head is [+human], it is more likely to encourage the acceptance of a pronoun reflex in a judgement task. None of the other studies quoted have controlled for this variable (though Tarallo and Myhill, 1983, note that [+animate] seems to favour pronoun reflex) Another finding is that only the beginners produce responses in all three

tasks that follow the Accessibility Hierarchy, and then with the exception of Subject and Direct Object Once again, performance is worst on the judgement task

The only conclusion possible at the moment is that the Accessibility Hierarchy seems to be a good predictor of frequency of pronoun reflex functions in free composition (cf Keenan, 1984) While it is true that the bulk of the evidence suggests that pronoun reflex insertion appears to be more frequent among learners with [+pronoun reflex] L1s, there is plenty of evidence to show that other learners also accept and produce them in a variety of tasks (See also Trevisi, 1978, for possible evidence of pronoun reflex insertion in the French of Italian learners) Since free compositions do not elicit a sufficient variety of relative clause types, experimental techniques must be utilised Quite the least reliable of these appears to be grammatical judgement tests, at least where the structures under test are embedded in sentences which may act as unwitting distractors We also need data from learners with [-pronoun reflex] backgrounds learning L2s where pronominal reflexes are the norm Here we would be forced to predict no difficulty in learning to insert such pronouns, at least on the basis of Eckman's claims, and in view of Zobl's (1980a, b) claim that English has a tendency to insert pronoun reflexes, at least in some non-standard dialects (but see Tarallo and Myhill, 1983) [13] At this stage of research, it is quite impossible to state one way or the other that pronoun reflex retention or insertion can uniquely be ascribed to the properties of a given L1 Methodologies need a great deal of tightening up in most respects, but most importantly of all, before we can even begin to study the phenomena in question, we have to establish the facts about the languages we are studying Schachter's insight about avoidance still seems unassailed As far as methodology is concerned, Bowerman's comments on the research on relative clauses in first language acquisition (Bowerman, 1979 294) apply equally well to L2 research

> .. it is unclear why different researchers have obtained different patterns of results, which in turn point to conflicting interpretations of how children process sentences containing relative clauses It is possible that children have at their disposal more than one strategy for handling multiple-clause sentences; which strategy appears dominant in a given study may be a function of the exact nature of the task, the scoring procedures adopted, etc.

1.6 Conclusion

The conclusion that may be drawn from the above is simple. There is crosslinguistic influence, and it exists in many forms. Sometimes the evidence for or against is controversial or open to reasonable doubt. Sometimes there are question marks about the methods employed to obtain data, whether they support the existence of CLI or not But whatever the merits of individual cases, the phenomenon is so pervasive that it cannot be considered peripheral to the study of second language learning And because it is so pervasive, it is important to establish precisely what the mechanisms are. There is now evidence to suggest that CLI not only acts as a source of surface form (thus leading to what used to be known as interference or positive transfer), but also serves to constrain the choice of surface forms in the learner's IL at levels beyond and within the sentence. Furthermore, where the adult L1 pattern is also consonant with a putatively universal developmental stage in L2 learning, the learner may proceed to that stage faster than those learners who do not have such a pattern in their L1s. Conversely, possession of the L1 pattern may lead to delay in the acquisition of the L2 target or even fossilisation.

It is misleading to say that CLI does not afflict the more advanced learner to a greater extent than the less advanced· this hoary old chestnut (planted by Taylor in 1975) should finally be squashed underfoot. It is based on very little evidence. If one only looks at the development of basic syntax or elementary morphology, then such an observation is both inevitable and trite Advanced learners are affected by crosslinguistic influence too, but in different ways from beginners because they know more and their knowledge opens up new areas of susceptibility.

NOTES TO CHAPTER ONE

1. This chapter contains some sections which are revisions of material which has appeared elsewhere. I would like to thank the Editors and Publishers of A Davies, C. Criper and A Howatt (eds), Interlanguage, Edinburgh University Press, 1984, for permission to quote from my chapter on 'The empirical evidence for the influence of the LI in Interlanguage' and the Publishers of E Kellerman and M. Sharwood Smith (eds), Crosslinguistic Influence in Second Language Acquisition, Oxford Pergamon Press, 1986, for permission to incorporate fragments from the Introductory chapter by M Sharwood Smith and E. Kellerman

2 This same point can be made to deal with Dulay, Burt and Krashen (1982:107), who argue that avoidance cannot be predicted by contrastive statements

3. Actually, parts of Chapter 5 of Dulay, Burt and Krashen are literal paraphrases of sections of Dulay and Burt, 1972

4 The ambiguity of the meaning of 'familiarity' is a further problem here

5. More strikingly, The Times recently carried an article by Richard Heller in which the following line appeared " .he should be told that alliteration was already vieux chapeau when Piers the Plowman knocked off for lunch"

6 It is also worth remembering that Hyltenstam's methodology, essentially gap-filling, allows maximum monitoring, being a discrete-point test In view of the critical remarks levelled at research using discrete-point testing, this fact has been surprisingly overlooked.

7. For a similar discussion, see Zobl, 1980a:473. Also Andersen, 1979, Jordens, 1977, and particularly Sharwood Smith, 1983

8. White (1985 56n) notes that in a production test there were indeed differences between Spanish and French subjects 'consistent with the view that the Spanish are carrying over the pro-drop parameter'

9. A pied-piped sentence like 'This is a book to which he refers as particularly relevant' is not grammatical, however.

10. [NP PP] is considered the unmarked one of the pair because of its greater productivity and on account of case assignment considerations.

11. Native speakers do not seem to agree with this claim, however.

12. For instance, roughly 60% of a number of Dutch first year university learners of English produced unacceptable translations of *zijn val werd door een boom gebroken* (his fall was broken by a tree) as a result of attempting to avoid the *breken-break* correspondence.

13. It is not, however, immediately obvious how the fact that English is a crypto-pronoun reflex language should contribute to pronoun reflex insertion by learners of English with [+pronoun reflex] L1s. See, though, Visser, 1963.

2. POSITION OF OWN WORK

2 0 Introduction

This chapter surveys a number of issues which are relevant to my general area of interest Section 2.1 deals with the use of retrospection and metalinguistic data in IL research; 2.2 discusses the relationship between such data and linguistic knowledge, in 2.3, I introduce the notion of constraints on CLI 2 4 is an examination of the variable influence of the L1 in IL development, while in 2.5, the final section, I conclude that there is probably no single process that can account for all cases of CLI

2.1 On retrospection and interview in second language acquisition research

The article which forms part of Chapter 3 first appeared in 1974 and was reprinted in 1976 It was conceived very much in the spirit of the writings of Corder (especially Corder, 1967, 1971b and 1973b). Corder represented a startling departure from most work on second language acquisition up to that time Working very clearly in a different framework from that of the traditional contrastive analysts, he was foremost among a growing band of researchers who saw the inadequacies of the contrastive approach (e g Wilkins, 1968; Selinker, 1969, 1971, 1972, Newmark and Reibel, 1970; Reibel, 1971. See also Chapter 4) Corder realised the implications of the new cognitivist paradigm for second language learning, with the learner being seen as a generator of hypotheses about the nature of the language he was learning based on the input he received Within this framework, errors could be seen as evidence for the kinds of linguistic hypotheses that learners formulated, and therefore they formed the principal data on which second language learning research was based. Error analysis thus became the most highly valued methodology. One further contribution of Corder's was to insist that within this new view, where the emphasis is clearly learner-centred, the word 'error' would have to go. Just as it was inappropriate to talk about children acquiring their L1 making errors, so it was inappropriate to talk about learners making errors

(Corder, 1971b). This view, never generally accepted because of the terminological difficulties it caused (see e.g. Zydatiss, 1974), served to underline Corder's essentially psycholinguistic standpoint as distinct from the dominant pedagogic one of the time

In his article "The elicitation of interlanguage" (1973b), Corder proposed that in order to enrich the array of possible elicitation techniques available to researchers to study the learning of a second language, one could use the learner as an informant in much the same way that native speakers could be used in field work or acceptability experiments Corder's arguments, explored in more detail in Chapter 3, were that the learner could act as informant about his own IL, while under the impression that what he was offering was information about the L2 For Corder, the learner could be considered a 'native speaker' of his IL. Furthermore, since the non-native informant was also a native speaker of an L1, he could provide 'authoritative interpretations' (Corder, 1973a:39) of his own L2 output and possibly provide metalinguistic commentary on it in a language with which he was totally conversant, particularly if he had been taught in a formal setting. Such an advantage was certainly denied the child acquiring a first language and quite possibly many native adults. All in all, some types of second language learners were particularly suited to provide intuitional data which, it was felt, might lead to a deeper understanding of the learning process by enabling researchers to go beyond text (with all its attendant problems of interpretation, selectivity and finiteness) and incorporate the learner into the process of analysis In other words learners could help researchers to produce grammatical descriptions of the IL which accorded with the learner's intuitions This process is what I have called latitudinalization.

As far as I know, mine was the first attempt to use interview techniques to explore L2 learners' grammars, and although I do not make great claims for it as far as results are concerned, I believe it to have been more than a useful exercise The transcripts while long and difficult to analyse, raise a number of interesting issues. For one thing, although IL intuitions had been elicited in other studies of the period, such as Zydatiss (e.g. 1974, 1977), such

studies were conducted with groups of learners rather than with individuals. For another group elicitation techniques were (and are still) almost always applied to material chosen by the researcher on the basis of some sort of prior analysis of learner material This means that learners do not provide intuitional data on their own output, but on an idealised, preselected, output The motivation for such an approach is clear - in this way, one also overcomes the inadequacies of finite texts as a source of data Furthermore, one addresses group problems rather than individual problems, which offers greater rewards to the pedagogically minded And finally, such forms of elicitation, usually formatted like grammar tests (and often multiple choice), are easy to score, and can be carried out in the written mode (For recent examples, see Kohn, 1986, Sorace, 1986)

There are disadvantages to such techniques, however For one thing, if the role of CLI is to be considered, they can only properly be used with groups of learners from the same language background. The second disadvantage concerns the inability of such techniques to take individual differences into account, nor do they take account of learner's feelings of certainty about and their consistency in the judgements they make (Sorace, 1985) This is particularly the case where multiple choice judgement techniques are employed, since the material presented to the learner is severely constrained by the researcher And an additional hazard with all forms of judgement task is that without careful instructions it sometimes seems to be the case that a sentence is judged incorrect but for reasons that have little to do with the structure under investigation (Kellerman, 1984 See also Chapter 1)

The interview suffers from none of these disadvantages, though it has plenty of its own It is, as I have said, more difficult to analyse, and it is extraordinarily time-consuming It is frequently unproductive from the researcher's point of view In order for retrospective data to be valid, the interview has to be conducted when the original text on which the interview is based is fresh in the learner's mind The biggest problem concerns the occurrence of post hoc rationalizations on the part of the learner, often, one suspects, born out of a desire to please the researcher In such

cases, the learner is inferring from his linguistic behaviour what processes were at work at the time, rather than actually recalling them. All these difficulties can be compounded if one is working with learners who do not share the same L1 as the interviewer.

Despite its obvious problems, if used as a 'discovery procedure' and not just as a means of testing hypotheses, oral elicitation techniques are vital adjuncts to other heuristic devices (cf. Zydatiss, 1977:40). One of the most useful aspects of such techniques are the insights one gains into what difficulty really is for a learner. It was my frequent observation that the learner's experience of difficulty often did not coincide with the actual location of an erroneous form More formal elicitation procedures of the kind used by Zydatiss and others cannot take this observation into account. A further advantage to the interview is that by concentrating on the learner's own production, the interviewer also allows for an error correction session. In such versions, we find that in some cases learners are able to correct their errors, and in other cases they are not, even though they are aware that they are erroneous In yet other cases they are convinced they are right, and will reject alternative, corrected forms. Such findings directly address the question of IL knowledge, and shed light on the theoretical distinction made between errors of knowledge and errors of processing/performance

Gass (1983) is also positive about the usefulness of intuitional data On the basis of an experiment with learners of different proficiencies she claims that learners begin with an ability to make general judgements about grammaticality, proceeding to an ability to locate the source of error in the case of sentences judged ungrammatical, and then on to an ability to correct the particular error in question. Sinclair et al. (1978:3) discuss the degree of explicitness of such statements, ranging from the least explicit, self-correction, via reflection on performance, to overt formulation of rules (cf. Chaudron, 1983:348, fn. 5)

Subsequent to the appearance of my paper, there appeared an M.A. thesis written in America by Schlue (1976, reported in Hatch 1978). Schlue recorded three university-level ESL students conversing with a native speaker, and then had the learners comment on their

grammatical awareness during the initial conversation according to the following categories· 1) no problems with a particular sentence, 2) recognised error and corrected it before speaking, 3) recognised error before speaking, but no time to correct, 4) recognised error after speaking, 5) recognised error after hearing the tape, 6) not sure if this is correct or not (Hatch, 1978·467). As in my research (see Chapter 3), Schlue found that her informants frequently chose category 1), whether an error was present or not, and category 5), although it is not clear whether the students also 'corrected' structures which were already correct in the original interview The remaining categories were rarely chosen Schlue reports that in those areas of grammar where progress was made during the course of her ten-week investigation, there was a shift in judgements from category 1) to category 5) and back again to category 1). Unlike Schlue, I found that uncertainty was a reasonably frequent concomitant of my informants' judgements, even where there was no error. I used five categories of certainty, though differently from Schlue These were a) sentence or clause definitely correct, b) sentence or clause probably correct, c) no strong feelings/don't know, d) sentence or clause probably wrong, e) sentence definitely wrong. My category a) corresponds to Schlue's category 1), my category c) to her category 6), and my e) to her 5).

Of the 116 sentences presented to 8 informants (each informant received only his or her own sentences), 32 8% were rated a), 34 4% b), 13.8% c), 9.5% d) and 9 5% e). Thus about two-thirds of the erroneous sentences were rated as definitely or probably correct. Having made students provide certainty ratings, I next asked them to compare a number of their sentences with corrected versions. Of the 32 sentences presented originally and given a) or b) ratings, 17 were now rejected Of the 9 category d) and e) sentences, 5 were now rejected This last seemingly paradoxical result is the outcome of a method of elicitation which assumes that the learner's difficulty and the occurrence of error must necessarily coincide. Consequently, a number of my 'corrections' focussed on parts of the learner's original sentence which they had assumed was correct. This meant that the part of the sentence which they were most uncertain about also appeared unchanged in my version, thus

informing them that their first intuition of incorrectness was not well-founded Thus, in illustration, the Francophone student who originally wrote

> I got tired of listening to him talking drivels
> (cf French bêtises)

and had rated it e), still preferred her version to mine

> I got tired of listening to him talking drivel

Her problem concerned the appropriate complementation following 'to be tired' - was it 'to listen'?, she had wondered The fact that my version did not change the complementation made her realise her first intuition had been correct

I now suspect that such feelings about correctness are at least partly due to the learner s personality, self-image and perception of the task (modesty is becoming even here, unless, perhaps, one has a professional interest in being correct)[1], and probably say less about the status of the learner s grammar than was originally conceived In some cases such feelings have to do with areas which learners with certain linguistic backgrounds are aware of as traditional sticking points Thus an advanced Japanese learner, whose English was excellent and virtually error-free, consistently rated her performance low because of her persistent insecurity with the English article system - an evaluation that was not entirely justified In the end, low self-rating will almost certainly mean that the learner is capable of locating the source of his difficulty

1976 also saw the publication of a study by Cohen and Robbins They too used interview techniques (with only three subjects) based on previously written material, and did indeed find learners able to provide seemingly useful explanation of their difficulties and errors One major problem with their research, however, is the fact that there was a considerable delay between the production of written work and the oral interview The written work was collected over the course of a single term It seems highly unlikely that subjects would be able to recall their thought processes at the time of writing, so that the danger of post hoc rationalization is very

real. Cohen and Robbins' conclusions, such as they are, are thus vitiated by their method. Furthermore, it is by no means certain that this sort of interview can be conducted with all kinds of learners. It is obviously most suitable for those with sufficient proficiency and a degree of grammatical knowledge. The criticism that only learners taught in formal settings can be used in this way should not be taken to mean that the application of such techniques is inappropriate. On the contrary, one makes use of whatever one can.

Another researcher who made early use of interview techniques is Glahn (e.g. 1980). She found that learners were able to provide detailed information on the linguistic problems they were facing and the means of dealing with them as they tried to complete an oral task. Glahn concludes that introspection is a valuable method from a qualitative point of view for gaining insight into the process of learning a second language For further discussion of the use of interview techniques in general see Perdue (1982 229 ff).

Part of the reason why interviews have not been more used resides in the difficulty of analysis and the labour-intensive nature of the work, as I have suggested Furthermore, there is the vexed question of the status of intuitions While it is recognised that a linguistic theory must also be able to account for linguistic intuitions, the status of intuitions has often been seen as essentially secondary rather than primary (e g. Snow and Meijer, 1977). Furthermore, serious doubts have often been expressed about the reliability of such intuitions in terms of what they really have to say about the underlying competences of speakers It has long been known that intuitional data can be manipulated by varying task conditions; a recent experiment by Carroll, Bever and Pollack (1981) demonstrates this most effectively. Furthermore, there are those who would maintain that it is very difficult to introspect about something so highly automatic as linguistic performance However, recent work by Kellogg (1980) and Ericsson and Simon (1980, 1984, in press) suggests that where the opportunity exists for subjects to reflect on specific problems, they can report accurately on their mental processes. That is to say, as long as focal attention is being devoted to the task, accurate introspection is possible.

Translated into our terms, the learner actually does what he says he does. Or the rule he applies is actually the rule he says he applies. Thus, as long as introspection techniques are used carefully with a number of built-in safeguards (see e.g. Poulisse, Bongaerts and Kellerman (in press) for these, and discussion of the value of retrospection in the study of *ad hoc* lexical strategies), they can be used as data of a sort, in conjunction with other data-gathering techniques which make use of intuitions, as well as the more normal methods of gathering L2 material which are less controversial. The introspection issue in second language acquisition has recently become live again after about a decade in the doldrums, with a number of researchers using it to study not only the learner's grammatical problems and how he sets about solving them (see contributions to Faerch and Kasper, in press), but also the processes of composition (Gaskill and Campbell, personal communications) and translation (Gerloff, in press).

The experience I gained from the use of oral metalinguistic elicitation supports Corder's suggestion that examination of learner text could be usefully supplemented by such techniques. Insights were provided into the sorts of problems encountered by learners which the more formal elicitation formats, for all their usefulness, could not provide. Clearly, the use of the two formats is complementary. It is also necessary in the development of more adequate descriptions of the learning process. Analyses based on text alone often lack conviction because there is no way of evaluating them in terms of competing solutions (see Chapters 1, 3, 7 and 8 for discussion).

2 2 The relationship between metalinguistic data and linguistic knowledge

While there are some researchers who do not see that the elicitation of intuitional data necessarily has any direct relation to actual linguistic performance in spontaneous situations where the emphasis is on language as a means of communication rather than as an object in its own right (e.g Faerch, 1984), many believe that such data do in fact have something to say about the L2 acquisition process (Corder, 1973b:40). The precise nature of the relationship

between the development of linguistic skills and metalinguistic skills is not at all clear (and is equally unclear for first language acquisition. See e.g. Hakes, 1980), though the use of intuitions as data about the process of second language is well established. (A survey and discussion of many of the studies using metalinguistic data can be found in Chaudron, 1983.) Even if the relationship between linguistic and metalinguistic development should prove to be tangential, the study of metalinguistic development would still constitute an area of research endeavour in its own right. Certainly in first language studies, researchers like Karmiloff-Smith (1979, 1984) have discussed language as a 'formal problem-space' for the child, to be grappled with and organised into a system irrespective of its purely communicative properties. I touch upon this question of language as object in the discussion of U-shaped behaviour in Chapter 8. We should certainly be aware that elicitation of metalinguistic judgements is part and parcel of the methodology of linguistic analysis, and as Arthur (1980) has strongly argued, the reliability of L2 judgements (actually judgements about the IL, as Corder points out) increases as proficiency increases. Thus, the reasoning goes, the more developed the IL grammar, the more reliable judgements will become.

Masny (1983), cited in Masny (1984), provides strong evidence that there is a relationship between metalinguistic awareness and specific L2 variables. She found that L2 proficiency, L2 achievement and L2 aptitude were the significant predictors of the ability of learners to make grammaticality judgements This seems to suggest no more than the intuitively pleasing fact that the better learner knows more language, but there is no *a priori* reason why a better learner should also be better at making linguistic judgements. This does not of course mean that metalinguistic awareness is anything more than an interesting by-product of the L2 process, but Morton and Marshall (1978) have argued that there is an important feedback role from this awareness to the normal unconscious acquisitional processes in the form of monitoring, control and repair. There is in any case a great deal of evidence to suggest that contact with a second language enhances language awareness (Cummins, 1978, Heeschen, 1978), even among children

(Burling, 1973; Sharwood Smith, 1981; Slobin, 1978).

Intuitions have been attacked because of their unreliability and their lack of obvious correlation with performance in spontaneous communication. Concentration on form, it has been claimed (e g. Tarone, 1985), leads to a characterization of the IL grammar that is the most removed from reality, since in such circumstances the IL is most permeable to outside influences which are not part of the grammar, such as the L1. Such a view is difficult to maintain, since it fails to take account of an important distinction between competence and processing. In fact, in Sharwood Smith and Kellerman (in press) it is claimed that CLI may occur either at the level of competence or processing. In the latter case, the learner may be unable to utilise his underlying knowledge since the relevant processing routines necessary to perform that knowledge may be inadequately controlled. In other words, grammatical knowledge may be underdetermined by performance, and it would be unwise to assume that the form of utterances is necessarily a direct reflection of underlying competence. In fact, in themselves, individual utterances are ambiguous as to whether they reflect competence or not (see Chapter 1).

2.3 Constraints on CLI

In Chapter 4, an elicitation technique is developed to deal with a specific problem. Whereas in Chapter 3, the intention is to bridge the gap between the mere collecting and classifying of errors and looking for explanations of them, here the concern is to investigate a phenomenon that had been observed in Dutch classrooms and elsewhere What triggered off the research described in Chapter 4 onwards was the informal observation of the occasional refusal by learners to accept certain L2 forms because they were stated to be too much like the learner's L1 or because they did not coincide with learner's beliefs about the appropriate meaning of those forms.

Levenston (1979:152) reports a similar phenomenon investigated by one of his students, which he terms "unreasonable homonymy". A Hebrew verb like /levaquer/ means both 'to visit' and 'to criticise', and this apparently unmotivated homonymy creates problems for non-native learners of Hebrew, who, like the Dutch

learners I study in Chapter 5 would seem to prefer single forms to have single (or at least tightly related clusters of) meanings (see Kellerman, 1981, 1983 for further discussion) There is clearly a degree of conservatism in some types of learner which acts independently of existing linguistic similarities or differences between L1 and L2 The observation that similarities between L1 and L2 would not always be exploited by learners was not a new one when first reported by Kellerman in 1977 (cf the examples quoted in Dulay, Burt and Krashen, 1982 104-5), and seemed to support those who believed that CLI had no role in second language acquisition However, since it was perfectly clear that CLI was a reality, the CAH would have to be held in abeyance or modified in order to account for cases where learners seemed to expect non-congruence Consequently it would be necessary to posit a model where the L1 could work variably in the development of the IL grammar Hence the notion of the 'strategy of transfer' in Chapter 4 Such a model of CLI would not only have to account for the fact that similarities were not always capitalised on by learners (that is to say there was no 'positive transfer'), since this had already been reported on in Dulay et al. (1982), but more importantly, it would have to account for what exactly in the L1 would not be capitalised on and why Dulay et al (1982) present merely a list of random examples, they cannot explain why the French learner quoted by Richards (1971) should say 'composed with' instead of 'composed of' in English (given French *compose de*), but then they do not feel the need to, since they do not believe in a role for the native language For them there is no *a priori* reason for a French learner to say 'composed of' However, my aim was to examine the issue of the constraints on CLI in a more systematic fashion Since my initial observation in the Netherlands had concerned idiomatic expressions, this is where my investigation began

The experiment reported on in Chapter 4 is essentially a judgement test requiring more active participation on the part of subjects than simply recording dichotomous responses Subjects were actually required to indicate exactly what they considered to be incorrect, if anything, in a stimulus sentence, and their corrections formed an important part of the subsequent analysis.

The rationale behind a methodology like this is simply that it will be difficult to collect idiomatic expressions in natural conversation, not only because of their generally low frequency compared to, say, articles or tense morphology, but also because, if our original observation is correct, L1-like idioms are not likely to be present anyway (cf. Irujo, 1984). We are dealing with a form of avoidance not caused by partial knowledge of the L2 equivalent, but by prediction of non-equivalence on the learner's part, that is to say by a prediction that idioms are language-specific An observation relevant here is that made by Henzl (1973), who noted that Czech native speakers edited out of their speech to non-natives the idiomatic expressions they used quite happily to other native Czechs.

As far as rejection of idiomatic expressions in a judgement task is concerned, we must assume that the input either has not contained the relevant expressions or, if it has, they have not become intake - that is, there has been no critical measure of positive evidence leading to changes in the learner's grammar. This is not to say that learners cannot interpret such idioms when they appear in the L2 (Irujo, 1984), though whether this is done top-down, as it would be in the native language (see for instance Ortony, Schallert, Reynolds and Antos, 1978; Swinney and Cutler, 1979; Gibbs, 1980; van Lancker and Canter, 1981) or via some off-line reconstruction process remains an empirical question. Too great a mismatch between literal and idiomatic interpretations of the surface form will no doubt be responsible for the rejection of L1-like idiomatic expressions in the L2. As Steinman (1973, quoted in Honeck, 1980) puts it:

> Speaking figuratively consists . . of saying (utterance meaning) what you mean (intended meaning) by not meaning what you say (sentence meaning).

The results of the experiments described in Chapter 4 confirm that learners cannot be seen as automata who simply transfer from L1. What makes the point even more strongly is that Dutch and English are typologically so close; given the undoubted incidence of CLI in the English of even advanced Dutch learners, it is significant that here we have an area where there is a clear

reluctance to expect correspondence between the two languages Subsequent research has tended to confirm the findings of Chapter 4, as we saw above in the findings of Sjoholm (1983) with Swedish-speaking and Finnish-speaking learners of English Huinen, van Oosterhout, van Roosmalen and Ruyters (1979), in an extended version of the experiment on idioms, found similar results, that is, a clear tendency to reject these expressions. Jordens (1977) reports that there is a statistical correlation between the estimated degree of semantic transparency of an L1 expression and the likelihood that it will be acceptable in the L2 His results also show a tendency for subjects to reject Dutch-like idiomatic expressions on the part of Dutch learners of German, though this was not apparent in first year university students, who were more accepting In Jordens and Kellerman (1981) we ascribe this difference in behaviour between students of English and German to differing perceptions of L1-L2 relatedness (or different 'psychotypologies' - Kellerman, 1983), with less experienced learners of German considering the languages close enough for equivalence to exist However, after a year's extra instruction, no doubt aided by translation classes, learners seem to develop a suspicion of equivalence, leading to the rejection pattern noticeable amongst learners of English at all levels

It was these findings that led me first to conceive of the notion of constraints on CLI While we may assume that learners expect idiomatic expressions to be a part of the language they are learning, it is also clear that they do not expect to find many formal correspondences with those in their L1, their special lexical (as distinct from syntactic) status effectively acting to make them language-bound (though this might be affected by language-learning/teaching experiences, and changing perceptions of typological relatedness) The apparent lack of correlation between the acceptability for the L2 of a given L1 subsystem on the one hand and the general closeness of L1 and L2 on the other showed that even in pairs of languages where there is considerable evidence of CLI, there are limits placed on the degree of CLI possible This is not to say that L1-like and incorrect idiomatic expressions would never be found in the IL of Dutch learners of English, but the probability

is low, lower than, say, the incorrect transfer of the meanings of L1 prepositions to the L2 'primary counterparts' (Arabski, 1979), as reported in Sjoholm (1983) Thus subsystems of the L1 would show variable tendencies in their ability to influence the L2 The research problem that arose out of this conclusion was how to establish the varying degrees of transferability of the various subsystems and how transferability could be predicted, since that attribution of language-specificity is essentially *post hoc* It was with this goal in mind that the experiment described in Chapter 5 was carried out Here, the object of study is the polysemous word pair, *breken - break* The question is whether it would be possible to account for learners' intuitions concerning the transferability of the range of Dutch meanings of *breken* to *break*

Two interacting constraints were hypothesised as being responsible for learners' intuitions about transferability The first, deriving from the discussion in Chapter 4, was that the closer two languages were perceived to be to each other, the more likely L1 features would influence the form of the L2 This constraint is referred to as the *language distance* or *psychotypological constraint* It is at variance with the classical CAH, which predicts that interference is directly proportional to the degree of difference between L1 and L2

The facilitating or restraining function of this constraint is tempered by the action of a second constraint, which has gone under various names in my work, such as 'coreness', 'markedness', and more recently, 'prototypicality' The first term is no longer used, since it suggests an underlying property common to a number of items or meanings This is the use made of 'core' in Caramazza and Grober (1976), who found that subjects could experimentally define five distinct senses of the word *line*, and that these senses could be ordered in terms of their closeness to an underlying 'core' meaning Furthermore, the use of the terms 'core' and 'periphery' invites comparison with the use of such terms in theoretical linguistics 'Markedness', while in itself a useful term, is plagued by conflicting interpretations in the second language literature, just as it is in linguistics The third term, 'prototypicality', seems free from the snags inherent in the use of the other two terms

The claim of this thesis is that the less representative of the prototypical meaning a usage of a given form is, the lower its transferability. The prototypical meaning of a polysemous verb will thus be the most transferable one - that is to say learners will naturally assume that the L2 formal equivalent of the verb (its primary counterpart) will first and foremost bear the prototypical meaning. If we assume that we can determine by quantificational means a scale of prototypicality for a polysemous lexical item, then the less prototypical the meaning, the lower the probability that this meaning will transfer to the primary counterpart selected to receive more prototypical meanings. As a corollary of this claim, the less prototypical a given meaning is of a given form, the more probably this meaning will be allocated to a different structure in the L2. This probability is what I understand by *transferability*. Transferability should thus be conceived of in terms of prototypicality scales projected onto an L2 by the learner. It is not dependent on the L2 of the learner being learned. The assumption of the existence of the transferability constraint is thus indirect, since its existence can only be inferred from strong correlations found between L1 intuitional data and data from learners who are also speakers of that L1 Only if the existence of a putative prototypicality dimension can be shown on antecedent grounds can it then be used to make predictions about the IL without invoking circularity. We thus arrive at the Transferability Hypothesis:

> Given that the learner establishes a correspondence between L1 surface form F and L2 surface form F', where F is polysemous, the more prototypical the meaning in the L1, the more likely it is to be attributed to F' in the IL

Unlike Eckman's Markedness Differential Hypothesis (Eckman, 1977), I do not claim that a less prototypical sense in the L2 is necessarily more difficult to learn than one which is more prototypical, if it is already instanced in the L1. It may only require a single instance of positive evidence in the L2 for it to become part of the IL grammar In this respect, the study of cognate languages is interesting, for even though Dutch, German and English are closely

related, learners underestimate, in some domains at least, the degree of isomorphism between them That they do so is in line with the suggestion of other researchers that learners do not always bring a fully-specified L1 to the task of acquiring an L2 Even in the case of a cognate pair like Dutch and German, the implication is that such universal processes as metaphoric extension are constrained by learners of an L2 to a set possibly more limited than required by either L1 or L2 Furthermore, this set is ordered, not random That is, it corresponds to a scale of prototypicality Once such a scale has been established, it should be possible to make predictions of a probabilistic kind about learner behaviour as long as the learners share a given L1

The idea that structures have prototypical meanings can be extended to idiomatic expressions, where we may speculate that a post-access decision procedure ascribes prototypical meaning to the literal interpretation of the structure in the L2 Furthermore, the discussion of *break - breken* also suggests that learners have notions about prototypical meanings of highly polysemous verbs In Chapter 6, an attempt is made to establish what factors determine prototypicality in the case of concrete anthropomorphic metaphors, in this case the word pair *oog - eye* The hypothesis that an interaction of subjective sense frequency judgements and 'similarity to prototype' judgements (in this case the body part itself) would predict (i e correlate significantly with) transferability judgements was to some extent borne out If these results are confirmed in further research, the importance of non-linguistic variables such as subjective frequency will complicate attempts to explain CLI in purely structural terms

2 4 The variable influence of the L1 in IL development

Chapter 7 represents an attempt to deal with the vexed question of what causes the persistent failure of Dutch learners to master the syntax of English hypothetical conditional sentences This subsequent work suggests that typical errors of the kind **If it would rain, the match would be cancelled* cannot uniquely be ascribed to the influence of Dutch, and that other factors will have to be considered as well One of these factors may well be the semantic

ambiguity of the past tense in Dutch and English, referring both to [past, real] (the prototypical sense) and [non-past, hypothetical]. Comparative data from other languages as well as historical evidence is offered to suggest that this error must be seen as evidence for a natural linguistic tendency where the role of the L1 may be viewed as catalytic rather than causal. This tendency may be responsible for the difficulty Dutch learners have in learning the English target, in that, in the L2, priority is again given by the learner to the prototypical meaning of a polysemous structure in the L1.

In the course of studying advanced Dutch learners' behaviour, I have been struck by what appears to be regressive behaviour on the part of learners. That is to say, more advanced learners appear to perform less well than less advanced learners in certain linguistic areas. This phenomenon has been reported before in the literature, but not in terms of U-shaped learning or performance curves. U-shaped behaviour refers to three distinct stages of development, where Stage One is characterised by behaviour which matches the target norm. Stage Two by contrast is marked by performance which now deviates from the target. Stage Three marks a return to the target behaviour. There has always been a fundamental assumption that learning is monotonic, a clear progression stage by stage towards the target, as shown by studies of the acquisition of negation. However, the Jordens (1977) study of idiom transferability discussed above is just one of a small number of second language studies which suggest that learner behaviour need not progress monotonically. Chapter 5 of this thesis also reports such a curve as far as intuitions are concerned Other second language studies with U-shaped characteristics are reported in Chapter 8 (see also Sorace, 1986). In first language acquisition too, the concept of U-shaped behaviour is relatively new (Bowerman, 1982) This sort of behavioural curve poses considerable problems for researchers using cross-sectional sampling techniques because a failure to entertain the possibility of such curves may mean that sampling with the last proficient or youngest group will suggest that the target form has been reached, a fact confirmed by sampling learners nominally more advanced who happen to be at Stage Three. Failure to note the existence of Stage Two will lead the researcher

to the not-necessarily-correct conclusion that the particular structure of interest has been mastered from the very earliest stage. Since the cases I studied in this thesis all concern target forms that are in fact identical to the native form (idiomatic expressions, senses of *break*, senses of *eye*, the (modal) past, etc., the existence of a second stage where the L1-like structure is rejected has considerable theoretical significance. Since one could now argue against Stage One non-deviance being the result of completed acquisition. Instead, this Stage might be seen as the one most prone to CLI. The existence of the intermediate stage shows how the role of the L1 is variable, acting both as provider and constrainer of linguistic form in the L2; furthermore, the existence of deviant Stage Two forms also means that other factors play a role in determining the development of the IL.

2.5 Conclusions

The general conclusions to be drawn from the above suggest that even in very close pairs of languages, learners do not bring fully determined L1s to the acquisition of an L2, and that specific psycholinguistic conditions have to be met for the L1 to exert influence on a developing IL. What I have not done (so far) is to specify in any detail what conditions have to be met with regard to the nature of the L2 as a trigger of CLI. It has, for instance, been claimed that the L1 may only play a role where the L2 is itself inherently unstable or opaque. Such a view, enshrined in Andersen's "Transfer to somewhere principle" (Andersen, 1983), is most recently expressed by Zobl (1984), who limits the role of the L1 to that of an "auxiliary evaluation measure" to be used in those cases where the L2 is typologically inconsistent or indeterminate, or where extralinguistic factors may determine the form of utterance, as in pragmatic word order. These arguments are seductive and thought-provoking since they provide the glimmerings of a means of predicting syntactic outcomes in IL and determining exactly when syntactic influence from the L1 will occur. However, I do not clearly see how they could be applied to the study of lexicalization patterns, any more than my approach could be used to account for syntactic phenomena, like the development of IL negation or word

order. I have tried to show that an approach to L1 influence in the IL such as I have espoused does at least allow us to make some tentative predictions about IL behaviour. Even if the L1's role was ultimately shown to be merely that of an auxiliary source of predictions when the L2 input in some sense fails, we would still have to account for the selective action of that source. And we would still have to account for the fact that L1-L2 similarities are generally capitalised on by learners. This is where the notions of 'prototype' and 'language distance' come in.

The approach outlined above has specifically addressed the problem of lexicalization of meaning in a second language. The principles proposed are relevant to this particular linguistic domain and not necessarily for syntax, discourse or phonology (though see James, 1983 who extends this framework to IL phonology). Lexis still receives too little attention in IL studies, and many people have implicitly assumed that the study of IL is in fact the study of IL syntax. Concentrating on a specifically delimited domain does not have to constitute a weakness of the approach, however. In fact it may be considered a strength. At this stage in the history of our young field it might be argued that it is appropriate to investigate specific domains in order to refine approaches with respect to those domains rather than continually rework the global theory of the day This 'modular' approach has been very fruitful in other disciplines, notably linguistics It does not imply a denial of the value of all-encompassing approaches but rather puts off the moment of trying to account for all of acquisition at once with a single set of principles Some of these principles may be extendable to other domains, and then perhaps only in adapted form. To what extent CLI occurs in similar ways in other domains is another question. I do not for a moment see why CLI should be a unitary process.

NOTES TO CHAPTER TWO

1 See for instance discussions in Dulay, Burt and Krashen (1982); van Els, Bongaerts, Extra, van Os and Jansen-Dieten (1984), and Hatch (1983).

3 ELICITATION, LATITUDINALISATION AND ERROR ANALYSIS[1]

3.0 Introduction

In this paper it is my intention to propose a broader and more exacting approach to the study of language produced by second language learners than has usually been the case till now In particular it is felt that the learner himself must be increasingly utilised in ways similar to those associated with native informants in order to provide the researcher with more data than are normally available by examination of learner text alone Thus by procedures aimed at obtaining both linguistic and metalinguistic information, a much more detailed picture of the learner's knowledge of his target language (TL) might be achieved

3 1 Problems with error analysis

Recently, the idea has been put forward (Corder, 1967) that the errors[2] made by language learners may be viewed as evidence of the learner's hypotheses about the nature of the TL, rather in the way that the child makes hypotheses in the acquisition of his native language (NL) Granted that this may be so, a study of errors should enable the researcher to explore some of the processes of second language learning in terms of the types of hypothesis formulated by learners, the order in which language items are learned, the effects of a teaching syllabus, the role of the learner's NL and other linguistic and non-linguistic factors which may have some bearing on the learning process To undertake studies of this kind, extremely complex longitudinal investigations are required and though these have been called for on several occasions (Corder, 1971a, Reibel, 1971, Richards, 1971), to date there have been very few actually completed (e g Ravem, 1968, Dato, 1971) This is hardly surprising in view of the considerable practical difficulties involved in carrying out such projects, both Ravem's and Dato's studies are based on the learning of a second language by their own young children

While the need for such longitudinal studies is clear, a great deal of valuable research has yet to be carried out on the

Latitudinal axis of the language learning process. This research would be aimed at examining thoroughly the state of the learner's knowledge of the TL at a precise moment in time by expanding the linguistic data base already available to the analyst from the evidence of the learner's text. If one confines oneself to such texts, then the data available for analysis are quantitatively impoverished, for there is obviously a limit to the amount of language that can be produced by a single learner, particularly in a classroom context, where time is inevitably in short supply. In addition, some of this language will be of limited value in studies of this kind it is difficult to see what use could be made of laboratory drills or blank-filling exercises (cf. Corder, 1973a). Certainly essays, reproductions, letter-writing, dictations, etc. can all serve as grist to the analyst's mill, for these are examples of language skills with applications in everyday life, especially where the learner is or has been a participant in further education. Nevertheless, the fact remains that despite the limitations in the amount of data deriving from this source, analysts have often felt confident enough to arrive at seemingly definite conclusions as to the provenance of errors. How, then, do analysts arrive at their conclusions about errors with such apparent certainty?

The answer would seem to have several strands. Firstly, the analyst as teacher will have experience of the problems generally associated with the learning of a particular language; he may share his students' NL or be a native speaker of their TL. He may also have the benefit of the formalised experience of other teachers He will certainly be familiar with the particular capabilities of his own students The analyst as linguist has recourse to the findings of contrastive analysis (which he may have to undertake himself) and to detailed accounts of the structures of his students' NL and their TL Accordingly he may bring considerable experiential insights to bear on the analysis of errors. Thus while on the evidence of a learner's output several hypotheses about the nature of a single error may be possible, all to some extent observationally adequate, the analyst will in fact tend, because of these experiential insights, to reject (or not even to formulate) any number of these. However, this is not to suggest that such experientially-assisted

hypotheses are never controversial. While such controversy may not be critical from a pedagogical point of view, it is clearly of some importance from a psycholinguistic one. There are times when it is very easy to quibble with some analyses even when they are expressed as if they represented incontrovertible facts.

An analysis which is occasionally open to the charge of unjustified self-conviction is that presented by Richards (1971) in a much-quoted paper. Richards, on the basis of very limited data, comes to conclusions that lead one to question the efficacity of analytical procedures based on such scanty evidence. To demonstrate what I mean, here are two examples drawn from Richards' paper. He proposes three categories of error; interference from the NL, overgeneralisation of TL rules, and performance (i.e. unsystematic) errors.

French speaker	*Richards' category of error*
... this is occurs	overgeneralisation

Richards says:

> "...the French speaker seems to have generalised the form *is occurs* from the experience of forms like *it is made of* and *it occurs*".
>
> (p. 16)

It is not at all obvious for what reasons he assumes that this confusion might have taken place, nor why he should have selected *it is made of* as one of the form types confused. Furthermore, he rejects the possibility of this error being an accidental formation (i.e. a performance error), as the student does not correct it on being shown the transcript of his text (cf. Corder, 1971b, esp. p. 152). Theoretically a performance error might be correctable without assistance but it is arguable whether it is sound experimental technique to ask a learner to check his own text for such errors, and subsequently to use the results of such a check as the means of identifying them. Surely a psychological phenomenon like a performance error cannot be defined on the basis that it is potentially correctable by its author. Not only is it easy for the learner to overlook such errors but it is also possible that he may

decide to alter a form he previously produced, not because it was now obviously erroneous to him but because he subsequently preferred an alternative at a point where uncertainty had existed in his mind as to the relative merits of two or more competing forms Such 'second thoughts' could not be operationally distinguished from corrections of performance errors In this context of 'subsequent correctability' it would be interesting to know how Richards would deal with a learner who 'corrected' an error-free sentence by producing an erroneous one in its place (I have had examples of this 'recidivism) In fact there are several possible directions of correction, for while we have seen that an erroneous form could be altered to an error-free one and vice-versa, it would also be possible for one erroneous form to be replaced by another, or a correct form to be replaced by another correct form Methodologically, all such alterations would have to be treated as performance errors - an unsatisfactory state of affairs It seems clear that where several possible explanations for an error exist, none should be given absolute priority on the strength of experiential insights alone

Here is the second example

French speaker	*Richards category of error*
The camera enregistrate the image	overgeneralisation
The image disappear	

Richards maintains that this error (and others cited in this article) is caused by the overgeneralisation of a rule that assigns zero endings to all finite verb forms, except, of course, in the 3rd person singular of the present tense in English The learner has thus failed to take account of this exception, according to this analysis However, this hypothesis cannot be considered very likely in this case for a number of reasons

a In the case of *enregistrate*, the correctly inflected form occurs just four short sentences later in the learner's text, viz.,

It is composed with a lens and behind the lens is little screen coated with cells and *enregistrates* the light.

b. This French student is capable of giving fairly intricate accounts in English of the workings of a camera, and elsewhere of the exploitation of natural resources and the principles of the steam engine. His command of vocabulary is clearly adequate, so that all in all it would be open to dispute that a student at his level of competence would make such 'elementary' errors except through inadvertence (i.e performance errors) The first example, it is occurs, may be seen to corroborate this argument for the main verb occurs is correctly inflected (though of course the verb itself is erroneous).

c The problem may be phonological These errors might well have been avoided if each piece quoted in the article had been written rather than spoken and recorded We may have here an example of interference from French syllable structure, though this hypothesis would not account for the correctly inflected *enregistrates* which occurs so shortly afterwards The occurrence of these two forms so close together lends weight to the argument against correctability as a defining characteristic of performance errors. I would prefer to think, using the little evidence there is, that *enregistrate* is simply a performance failure, a psychological slip compounded by phonological pressure from French

On the face of it, Richards' classifications do not seem convincing because they are insufficiently supported Similar statements as to the provenance of errors are not uncommon. Buteau (1970), for instance, in her analysis of errors made by students of French, attributes the selection of the forms *finissont* and *venont*

in the frames *ces élèves__* and *tes amis__* in a multiple-choice test to the generalisation of "the inflection used in the present tense of *avoir*, *être*, *faire*, *aller*, and in the future of all verbs" Her assumption thus is that this handful of irregular verbs could interfere with a rule of nearly absolute generality in French, namely that the third person plural present tense ending is *-ent*. Furthermore the morphology of the future tense is also somehow involved, though no motivation is given for this attribution How she can safely make this assumption on the basis of an incorrect selection by a relatively small percentage of students (12 3% and 5 8% respectively) in a multiple-choice test is not at all clear In Dulay and Burt's paper (1972), it is maintained that the non-appearance in interrogative sentences of auxiliary *do* with full verbs in the speech of Norwegian children learning English was due to their overgeneralisation of *have + NP ...?* while living in England, "where the verb *have* is permuted *Has he a job? Have you a cold?*" This sweeping statement about *have* in British English is manifestly inaccurate (cf Quirk, Greenbaum, Leech and Svartvik, 1972, p 73)

It is refreshing to come across analyses which have been thoroughly researched (cf Bickerton, 1971, Hill, 1957) or which at least strike a welcome and appropriate note of caution Duškova (1969 16), in her analysis of errors made by Czech learners of English, makes a very good point about inferencing from limited data

> 'For the purposes of teaching, nonce mistakes appear to be of small value since the conclusions that can be drawn from them, if any, apply only to one particular learner, and *unless some system can be discovered in them*, they are of little value even in the case of the learner who commits them
>
> (italics added)

Instead she proposes that the analyst should concentrate on recurring and systematic errors made by a number of learners This indeed may be said to represent the traditional approach to error analysis, and certainly from a pedagogical point of view, it would be uneconomical to do otherwise However, a psycholinguistic study of second language learning requires the analyst to look more carefully at individual outputs in order to discover what system or

systems, if any, are operating behind the most minimal data Again it is necessary to reiterate the need for latitudinalisation of available data, in order to refine the first-order hypotheses that come as a result of applying experiential insights to these data. A considerable degree of latitudinalisation can sometimes be achieved by elicitation of specific language items from learners. The subsequent increase in relevant information might enable the analyst to evaluate more clearly the competing claims of various observationally adequate and experientially valid hypotheses.

3.2 Using the learner as informant

The procedure aimed at latitudinalising data which I am suggesting is hardly controversial and differs little from data gathering and analysis procedures outlined by linguists such as Garvin (1974). The first stages follow closely those of conventional error analysis except that they apply to individual learners rather than groups and can be used to deal with nonce forms. Thus the initial step consists of the selection of the appropriate texts from which the analyst will then isolate those areas which are of particular interest to him On the basis of a preliminary analysis he will arrive at a number of first-order hypotheses carrying varying degrees of conviction. These hypotheses he will then test, in the first instance by constructing errors based on the 'model' error contained in the learner's text. These constructions will be so formed as to incorporate the first order hypotheses, and will then be fed back to the learner for his approval or rejection. The learner thus provides the analyst with judgements about the nature of his own internalised TL grammar. In this way it might be possible, on the basis of the learner's reactions to these test forms to modify, reject or confirm the first-order hypotheses. This will hopefully lead to an abstract characterisation of errors rather than a mere listing of them. By using the basic techniques of elicitation, such as syntagmatic and paradigmatic variation, paraphrase, translation, etc , the amount of specific data obtainable may become considerably enriched There is no reason why, through elicitation, latitudinalisation cannot

continue into areas less directly related to items in the original text, since it is no use waiting for the evidence to appear textually, not only because there is no guarantee that it will ever do so, but also because the learner's language is presumably in a state of flux. Dilatoriness in the elicitation stage will tend to invalidate attempts to achieve expansion of the data base To attempt to discover something of what the learner knows, the analyst has to work quickly in order to 'freeze' that knowledge at a given point (cf Reibel, 1971, esp, p 95n)

Corder (1973b) has pointed out many of the ways in which second-language learners as informants differ from their counterparts in other fields, i e the child acquiring his first language and the adult native informant These differences may be summed up as follows very young children possess only what they know of their first language, the language learner on the other hand, if an adult, not only knows (or thinks he knows) something of the TL but also possesses a NL Furthermore, if he is an adult who has been the recipient of language teaching he will almost certainly possess a metalanguage of some sort as a by-product of that teaching Very young children are not generally considered to possess a metalinguistic faculty (though a recent study, Gleitman, Gleitman and Shipley, 1972, suggests they might do) The native informant may possess a metalanguage and speak the researcher's NL, but not necessarily so Additionally, the native informant's judgements about his language may not correlate with his actual language performance, for during elicitation he may be appealing to some supposedly higher linguistic norm to which he aspires but which he does not attain (cf Samarin, 1967, p 196) This is far less likely to be true of the second language learner, particularly when his text forms the basis for elicitation Corder rightly points out that the possession of a metalanguage is important, for with it a learner can make available to us his intuitions about the nature of the language he is learning However, the existence of such a metalanguage does not necessarily mean that the learner's intuitions are valid, for it is unlikely that any learner (let alone native speaker) is capable of giving a complete account of them, and often such accounts as there may be represent little more than post-hoc

rationalisations on the part of the learner as a response to questioning by the analyst.

These metalinguistic statements cannot be ignored, however, for they can provide clues about the way in which the learner's language is organised when conventional analyses fail or are insufficiently refined. There is no reason why a learner cannot be asked to give an explanation as to why he arrives at a certain form, and it is often interesting to see whether the analysis arrived at by the researcher matches the explanation offered by the learner. The danger exists for the learner, however, that once such an explanation has been voiced, he will follow it to the letter, even though the 'true' linguistic rules he has been applying previously are not really those implicit in the learner's statements to the analyst. However, when so little is known about what the learner does, *post-hoc* rationalisations are potentially useful but they have to be viewed circumspectly. I give two examples where such statements have interesting implications. They were made by two students during discussions of particular sentences each had written. Neither student knew that each sentence was, in fact, erroneous. The first student, German, wrote:

> The boat was going to leave Ostend harbour at 13.30 the other day

when her co-text and translation of part of the sentence as *am anderen Tag* unambiguously indicated that she should have used *next* in place of *other*, i.e *the next day*. On the basis of experience, one might attribute such an error to interference from German. However, this analysis seems to be an oversimplification Firstly, the student was convinced that her sentence was correct English, at the same time rejecting another sentence containing *next* in place of *other* in favour of her own, even though she quite willingly conceded that one could say *am nachsten Tag* in German. This suggested that she may have had some sort of idiosyncratic system which distinguished lexically between temporal and spatial relationships or between *proximity* and *succession*, or combinations of both Elicitation along these lines was not entirely conclusive though the temporal/spatial distinction seemed to exist. I then

asked the student what the difference between *other* and *next* was and the answer was a confirmation of the temporal/spatial distinction [3] Since elicitation had already indicated that such a distinction was probably being made, the student's statement can be assumed to have some foundation. This example also goes to show once more how analysis of text alone may prove inadequate, even though subsequent analysis serves only to make the situation more complex.

The second example relates to a Swiss-German student who wrote the following:

I had not a great conversation

After elicitation aimed at exploring some of the syntactic implications of the *have* form, I asked the student for her comments on the equivalent English sentence containing the dummy auxiliary *do*, viz ,

I didn't have much conversation

Again this sentence was rejected in favour of the original erroneous sentence She offered the unsolicited statement, however, that although she had often heard sentences with *do + have* in England, she had never used them, as her teacher in Switzerland had told her that main verb *have* never took *do*-support. This apparent case of one rule for the English, and another for Swiss students tells us at the very least that the student was prepared to justify her sentence in the face of my probing. Whether there is any truth in what she said cannot, of course, be established.

There is one further area where the learner can offer the analyst some direct assistance, for he can give an account of the relative difficulty he experiences in the production of a particular form. He is often able to state not only the precise point at which the difficulty exists, but also what alternative forms were available to him (if any) and why they were not selected. Furthermore he can state whether such processes as analogising or inferencing (Carton, 1971) were available to him. It is apparent in work of this kind that the occurrence of error and those points at which the learner experiences difficulty do not necessarily coincide, for an error-free sentence may have caused the learner considerable difficulties in its formation while a completely erroneous sentence may have caused him none. Thus learners often

demonstrate just how strongly certain patterns are embedded in their internalised TL grammars by total adherence to them during elicitation. The following sentence was written by a German student who maintained that this sentence was correct English even after being asked to comment on the correct form with *at* replacing *in*

but in that very moment it was six o'clock

The following short conversation then took place

EK Now you thought you were 100% right I wrote *At that very moment.*
You are quite convinced that it is *in that very moment?*

S The only question would be, is it correct to say *It was six o clock* or *It had been six o clock.*
That would be the only question

Here we see one of the more fortunate (if serendipitous) side-effects of such elicitation procedures in that occasionally one obtains useful information when it is not expressly being sought In this case we learn that there is a trace of uncertainty as to the selection of simple past over past perfect On the basis of this statement, one might explore this student's knowledge of aspects of the English tense system

Other examples of such adherences to erroneous forms are not difficult to find, and, not surprisingly, they often seem to reflect interference from the NL I append one more example of such adherence, which additionally gives very limited evidence for the learner's tolerance of the positional variation of a subordinate clause within a sentence The sentence in question is

Arrived in Dover, the first thing what I had to get used to was left-hand driving

Again as in the previous example, the student's expressed difficulty lay not with the erroneous parts of the sentence, but with a part that was actually correct, namely *I had to get used to*. The dialogue was as follows

EK You said you were unsure about this sentence. What were you not sure about?

S: This part of the sentence, *I had to get used to...* This was a little bit much, but I tried it.

EK: You thought this was probably where you were going to be wrong?

S: Yes, it could be, yes.

EK: ...Would you tell me if this sentence is for you correct English? *The first thing what I had to get used to, arrived in Dover, was left-hand driving.*

S: No, I think you must put *Arrived in Dover* at the beginning or at the end.

EK: So you would say *The first thing what I had to get used to was left-hand driving, arrived in Dover.*

S: Yes, I think you can say it.

EK: Would you say that this sentence is right? *Sat down, I asked for a drink. You're in a restaurant or something.*

S: Yes, I think so.

EK: Now try this one. Still in the restaurant, right? *Eaten the sandwich, the waiter brought me another.*

S: Yes.

etc.

Of course, one must treat the data derived from such elicitation with some care because of the effects of perseveration, amongst other things. Elicitation sessions incorporate a *teaching effect*, and the longer an informant is asked for his judgements, the less likely these judgements are to be reliable.

While the above examples seem to demonstrate how committed a learner may be to his own production (with the reservations as to the reliability of such data already mentioned), learners tend to display varying degrees of commitment ranging from total confidence to a complete lack of it. These degrees of adherence may be due, in some respect, to individual personality traits, and it is

unfortunate that it is extremely difficult to check up on the findings of conventional and lateral analysis by observation of the spontaneous speech of learners Nevertheless, it would be wrong to accord all errors equal status within the learner's grammar Clearly some errors do represent absolutely correct forms for their authors but others may represent forms in conflict with competing forms The degree of conflict between two or more competing forms might be expressed in theory by a score of probability of a form actually appearing. Thus while groups of learners with the same linguistic background may be said to produce errors described as typical for that group, this does not mean that these errors are in any sense equal in terms of each individual's TL grammar

To take extreme cases, a meticulous psycholinguistic description of the learner's language should attempt to distinguish between a systematic error and a performance error, when both are identical in surface form, and would be described in identical ways in any formal grammatical model. Psychologically, and pedagogically, they would be quite different, of course. A possible example of the former type of error is, as we have seen, *the other day*, instead of *the next day*. At the same time this very error was made by another student, who without prompting was able to correct her sentence and offer the explanation that her error was due to carelessness, and that *the other day* was 'a very German translation'. There will be other types of error too, some limited to production only (i e the learner recognises the correct form even if he does not produce it) and there will be those errors caused by the learner exploring the TL. It does not seem that it is methodologically possible at the moment to distinguish confidently between these (and other) types of error, but patterns of response in elicitation procedures may provide clues as to the possible status of such forms Rules could then be written for fragments of the learner language, incorporating, in the relevant areas, 'shadow grammars' in which alternative forms might be expressed as variational probabilities In some cases the shadow forms might never appear, of course, even though they might continue to interfere with the production of the selected form It is likely that the variational probability of the principal and the shadow form will alter vis-a-vis each other as

time goes on, until the latter is extinguished. Thus it is unwise to assume that the appearance of an ostensibly correct TL form indicates that the learner has at least in one area no more to learn, for not only might such a form be 'right by chance' (Corder, 1971a) but it might be in competition with any number of competing forms up to and including free variation.

My own work follows the methodological proposals above as far as is possible Thus while the procedures leading to the formulation of first-order hypotheses are reasonably straightforward, the practical problems of elicitation of linguistic and metalinguistic data are considerable. Each step of the procedure is time-consuming, and for this reason, and in order to minimise the time-factor, preliminary analysis may be sketchy. A sketchy analysis leads to sketchy elicitation, with the result that one finishes up with incoherent data and desperate appeals to the learner for explanations. Here lies the basic problem of all attempts at latitudinalisation; any set of procedures designed to collect and analyse data is really longitudinal. Accordingly one must make a methodological assumption, namely that provided the analyst works swiftly to complete the various steps, he is effectively examining a latitudinal section of the learner's knowledge. How swiftly he has to act I cannot say - I aim to complete the various steps and procedures with an individual within one or two days. However, though I believe that elicitation has an important role to play in such studies of second language learning, I do not believe that we can do much more than begin to scratch the surface with such procedures. One needs considerable luck, and it is easy to miss potentially useful important lines of inquiry during the course of the investigation. Unfortunately, it is not always possible to return to the learner to pursue these lines because of changes in the state of the learner's knowledge which may have taken place, or, quite simply, because he is no longer available

In order to make advances in the study of the language learning process, we need to know more than the nature of linguistic inputs and outputs. Clearly the beliefs of the learner himself about the specific language he is learning, whether these beliefs are the result of teaching or derive from the learner himself, must affect

with various degrees of subtlety the actual language he produces. Differences between native and target cultures may also have some significant part to play Although measures of attitude and motivation do exist in the field of second language learning (cf Jakobovits, 1970) these are likely to prove insufficiently detailed or revealing. More sophisticated methods of research are needed, designed to access the personality of the individual, especially within the actual teaching/learning situation itself. The reaction of teacher to student, and to the class as a whole, as well as the reactions of the students to each other must have some effect on the learning of the target language Observation has shown that, all conditions being equal, while some students attempt to produce extremely complex work both in content and language, others will usually be satisfied to produce work which, though error-free, is relatively simple in content and structure In the first case it seems as if the student is putting the desire to communicate above the desire to be grammatical. This difference may be attributable to individual characteristics not limited to the learning of a second language; on the other hand, it may be the result of a particular approach to the learning of the second language The analyst needs help from the social psychologist in devising schemes by which such information might be obtained. For the moment it seems unlikely that very much can be done in this direction and even if such schemes were available it is not at all clear how their findings could actually be linked up to linguistic performance. Obviously a start can be made through the application of certain tests (perhaps measuring introversion/extroversion, dogmatism, conservatism, extra- and intrapunitiveness etc) to individual learners, coupled with in-depth interviews in which a picture of the learner and his varying beliefs and attitudes might be elicited. Such interviews would, however, require an inordinate amount of skill on the part of the interviewer As for the role of the individual in the teaching/learning situation, one method that springs to mind that can be used to assess the individual's standing within the group is Interaction Process Analysis (Bales, 1970) which makes use of the intuitive feelings that people have for other members of their group, and also makes use of the skilled

observation of the group in action For the moment, however, the fact remains that what we know of the learner's contribution to the language he produces is limited to impressions. We need an overall, accurate picture. Coupled to this, it would also be necessary to have access to the material which served as input for the learner, i.e. the actual teaching he received. Knowledge of this material, which does not necessarily constitute the intake itself, would allow the analyst to make some inferences as to the individual strategies adopted by learners, but the practical difficulties remain enormous

Until we know a great deal more about the psychological and social make-up of individual learners and the strategies they bring to the problems of learning, we can make little more than informed guesses as to what language learning mechanisms are. Nevertheless I maintain that by bringing the learner into the analysis of his text in some of the ways outlined in this paper, we are making available data that are useful in the refinement of techniques of analysis of language produced by learners beyond the point permitted by the application of experiential insights alone

NOTES TO CHAPTER THREE

1. This is an edited extract of a paper published in 1974 in York Papers in Linguistics, 4, pp. 165-89 under the title Elicitation, lateralisation and error analysis. Reprinted in Interlanguage Studies Bulletin 1, pp. 79-114, 1976. The term latitudinalisation has been used here to avoid confusion over the meaning of lateralisation.

2. Throughout this paper I use the term error to refer to those forms which teachers of EFL would be expected to judge as incorrect. The term is retained despite its prescriptive flavour because the students on whose data parts of this paper are based have all been working towards advanced EFL examinations. It is recognised that while on the one hand there are those errors made by learners that nearly all educated native speakers would consider unacceptable, on the other there are those forms about which there would be considerable disagreement. In such latter cases, teachers still have to decide whether to allow these forms, which though to some extent acceptable to them, they themselves would never teach to students.

3. Her explanation was as follows: "The other day is a question of time ... I speak from today, and I can say The other day, that I can say but Next ... Near to me, I would say Near to me".
(Position?)

That's it. Yes, that's it. Other is distance in the sense of time, and Next to me, it is Close to me.

(Today and Tomorrow, this is Other?)
Yes.

4. TOWARDS A CHARACTERISATION OF THE STRATEGY OF TRANSFER IN SECOND LANGUAGE LEARNING[1]

4.0 Introduction

Interference as a phenomenon in second language learning has been extensively discussed in the literature of applied linguistics, especially since the appearance of Lado's elaboration of Fries (1945) in *Linguistics Across Cultures* (1957). Even though the psycholinguistic theories current twenty or so years ago have now largely been superseded, there has been no significant challenge to the notion that learners of a second language are prone to incorporating native language (NL) features in their attempted target language (TL) production, leading to a particular class of errors easily ascribable to their source by means of a bilingual comparison of the learner's attempted TL utterance and its equivalent in the NL.

This paper begins by reviewing two competing approaches to transfer as exemplified by the Contrastive Analysis Hypothesis (CAH), and by Newmark and Reibel's notion of *ignorance*. Subsequently a new view of transfer is presented. transfer is not to be seen as a strategy which a learner can use to compensate for a lack of specific L2 knowledge. However, there are constraints on the use of transfer as a strategy, relating a) to the learner's perception of NL - TL distance and b) to the particular nature of the NL item itself. If the NL and TL are typologically diverse and/or the NL item is considered *language-specific*, transfer will not necessarily occur. Some experimentation is reported which shows that Dutch idiomatic expressions are generally not treated as transferable to English (irrespective of whether equivalents do exist in that language). However, whether Dutch learners of German treat such expressions as transferable to German depends on their level of proficiency, with less proficient learners being more willing to transfer. These results are discussed within the theoretical framework expounded in the earlier sections of this paper, particularly with regard to language distance

4.1 The Contrastive Analysis Hypothesis

One great source of controversy relating to interference is the status of what has been termed the *Contrastive Analysis Hypothesis*. Lado believed in the predictive power of CA - predictive in the sense that if the linguistic systems of NL and TL were rigorously compared, it would be possible to draw up a list of difficulties that learners would experience in the TL

A knowledge of such difficulties on the part of teachers and course designers would lead to the development of efficient teaching materials aimed at overcoming the difficulties predicted, without wasting unnecessary time on those parts of the TL which were 'the same' as the NL Stockwell, Bowen and Martin (1965) go so far as to propose a hierarchy of difficulty deriving from a CA of English and Spanish They write

> "Assignment of an item (in a hierarchy of learning difficulty) is based on the premise that (positive) transfer from one language to another . becomes more difficult as the correspondences weaken".
>
> (p. 292)

Instead, they maintain, the growing lack of correspondence will lead increasingly to the likelihood of interference It will be seen that the term *difficulty* takes on rather special meaning within this framework, namely *degree of linguistic difference* It is a measure based on formal descriptions of units selected by the linguist for comparison; this measure, for some rather unclear reason, then appears to assume a psycholinguistic reality of its own. This presupposes that a linguistic description of differences is also a description of how the learner behaves, i.e that linguistic and psycholinguistic units co-incide, and further that the cross-lingual identifications made by the linguist (which are abstractions from behavioural events or intuitively arrived at) are also relevant to the behaviour of the learner when confronted with the TL (cf. Vildomeč, 1963.19).

The whole notion of a hierarchy of difficulty was an inevitable outcome of Lado's work, the relevant quotations from which are so

well-known that repetition will be restricted here. Lado's *fundamental assumption* is that:

> "individuals tend to transfer the forms and meanings and the distribution of forms and meanings of their native language to the foreign language ...".
>
> (1957:2)

This *fundamental assumption* is never explicitly supported by any reference as to the relevance for second language learning of the psychological theory on which Lado's tenets are presumably based. One supposes from what little psychological discussion Lado engages in (e.g. 1957:58) that interference comes about as a result of *proactive inhibition*, whereby a series of responses already learned (the NL) tends to appear in situations where a new set of responses (the TL) is required, given the same 'stimulus field'. Prior learning interferes with subsequent learning, at least initially, until the habit strength of the second set of responses is such that the earlier set no longer interferes (cf. Jakobovits, 1970:194). However, the connection between transfer experiments in the laboratory (which is the place where the term *interference* strictly belongs) and transfer in second language learning has been shown to be very tenuous, with many writers being reluctant to link the two in any significant fashion. Jakobovits (1970, p. 190) is particularly pessimistic, quoting Crothers and Suppes' (1967) findings. He criticises attempts to extrapolate from laboratory experiments on the grounds that these are based on "materials of an entirely different order of complexity" Nemser and Slama-Cazacu (1970), in a partial review of the literature of transfer, describe the concept of *transfer-interference* as controversial, while Wenk (1974) concludes that the term *interference* can mean all things to all men.

Dulay and Burt (1972) also attack Lado on sociolinguistic grounds, claiming that his references to Weinreich's and Haugen's writings in support of his assumptions are misguided (Weinreich, 1953; Haugen, 1953). Weinreich and Haugen were, after all, not concerned primarily with language teaching, but with bilingualism in contact situations, so that extrapolation from their work to CA is unjustifiable, say Dulay and Burt, on two main counts:

(a) neither Weinreich's nor Haugen's definitions of interference ('linguistic borrowing' in the latter case) presuppose a direction of transfer from first learned (NL) to second learned (TL), but rather a tendency for patterns from one language to appear in another as the result of contact; if anything, it is the language being learned which affects the NL (cf. Haugen, 1953:363). Both Weinreich's and Haugen's evidence derives from data relating to linguistic communities which have been 'transplanted', e.g. Norwegian communities in North America. CA, in the form proposed by Lado, on the other hand, deals with foreign language instruction in the classroom.

(b) the bilingual speaker may deliberately introduce loan translations "for the sake of enriching his language" (Haugen, 1953:459); the inference is that transfer can be socially motivated. According to Lado, interference is the unwanted reflex of the collision of two linguistic systems.

If the theoretical basis on which CA rests is shaky, in practice it has been shown to be a poor forecaster of learner behaviour. Corder (1967), Strevens (1969), and Wilkins (1968) are three writers who have maintained that CA could neither predict nor account for a number of characteristic errors made by learners, where the source is apparently the TL itself. More critically still (since TL-based errors are not really the province of CA, however much Lado claimed for it), practical CAs have failed even to do the job they were set up to do, namely to predict 'difficulties' (in the form of errors) caused by transfer of the NL. Whitman and Jackson (1972), in testing four CA's, conclude that "what results there were are quite lackluster" (p. 40), and that CA, in the context of their findings at least, was inadequate. Similarly, Sjöholm (1976:97) found CA to be inefficient in predicting difficulties in areas of English for Swedish and Finnish learners. (For arguments in favour of a predictive CA, see Schachter, 1974).

4.2 Formal and efficient causes of interference[2]

Any theory of second language learning which attempts to account for interference must recognise that there are two components in the analysis of data which must be kept rigorously apart. Firstly there is the phenomenon of linguistic interference itself, visible in the errors made by learners with given NLs. Secondly there is the mechanism behind that phenomenon, the psychological process known as transfer whereby prior learning is carried over into new learning situations. We thus need to make a distinction between formal and efficient causes. The formal cause of errors in second language learning is the NL, in so far as analysis is restricted to the linguistic product The learner's erroneous sentences in the TL and their NL equivalents are similar or identical, so that on formal grounds only, the NL is the source of the TL material. This linguistic identification has no psycholinguistic consequences, and though it may be interesting as evidence for transfer, it is not proof of it, nor does it explain why or how it occurs. Formal causes of error are, of course, extremely important to those concerned with analyses of material produced by learners. Linguists (and some would claim that CA is the province of linguists by its very nature, e.g. James, 1971) are interested in systems and comparing them - errors may provide a pretext for doing this. And teachers, faced with the prospect of evaluating their students' production, must be able to pinpoint the error and bring into action whatever remedies they consider efficacious Psycholinguists, too, whose interests lie deeper than formal causes, must initially derive their hypotheses from the observation of language data. From these observations, it may be possible to infer the efficient cause of interference, the mechanisms or processes by which the learner uses his NL in the TL.

It is precisely this understanding of the difference between formal and efficient causes which allow Dulay and Burt (1972) to posit non-NL sources for apparent interference errors [3] They propose that as far as prepubertal children are concerned, NL and TL learning processes are qualitatively the same and mutually independent so that TL errors will not be caused by transfer from

the NL. Instead the authors reinterpret errors whose formal cause could be the NL as developmental, i.e. similar to errors made by children acquiring their NL. Thus if the learner of English with Dutch as NL produces:

I think that he doesn't come

we might attribute the form of this sentence to the influence of Dutch where raising the negative from constituent to matrix sentence is optional, or we might refer to evidence from child language acquisition studies and find that sentences of this kind are also produced by children.

He thinks he doesn't have nothing
I think it's not fulled (sic) up to the top
I think I don't know what it is
I think he doesn't like us no more

(Bellugi, 1967, quoted in McNeill, 1970.93-4)

And so in the same way the Dutchman's sentence may be seen as developmental and independent of the influence of the NL. By describing these errors as interference-like goofs, Dulay and Burt recognise that linguistic data ought not to be taken just at their face value. They try to show that these errors do indeed have their counterparts in the literature of first language acquisition and are in fact examples of such processes as generalisation of the linguistic rules of the TL beyond the limits compatible with a fully-formed adult grammar.[4] Such utterances as:

Now she's putting hers clothes on
She's gonna brush hers teeth
Hers pyjamas

made by a Spanish child, which might be thought to show the influence of Spanish modifier-noun-number agreement, are, according to Dulay and Burt, due to the generalisation of an English rule governing the use of certain NPs possessively, as in:

Tom's books
The Prime Minister's statements

Furthermore, in support of their hypothesis, these authors did not find such structures as Bigs houses or Talls boys, which, they say, would have been clear cases of interference, as predicted by the mechanical application of an orthodox CA. While on the limited evidence presented, this analysis cannot be ruled out, the burden is still on the authors to show that this sort of error can be made by speakers with NLs where number agreement does not follow the Spanish model. One would also require evidence from the occurrence of non-agreement of modifier with singular nouns as Hers book, Hers tooth etc. (cf. Mary s book).[5]

Another criticism of Dulay and Burt's paper is that they simplify unduly and thus call into question some of the cornerstones of their argument. Thus, in examining some of the utterances produced by a child speaker of Norwegian learning English[6] they seem to misrepresent the facts about British English In this case they attempt to show that the Norwegian-like·

Drive you car to-yesterday?
Like you ice-cream?
Like you me not?

do have their counterparts in American English. However, this child was exposed to British English, where, according to Dulay and Burt:

> "the verb *have* is permuted
>
> *has he a job?*
> *have you a cold?*
>
> Since *have* is also a verb of very high frequency with children as well as adults, we hypothesise here the generalisation of *have + NP ...?* to other verbs. This goof (error) did not appear in (NL) acquisition data, perhaps because the children studied were American and there is less possibility of being exposed to the structure *have + NP ...?* in the U.S.".
>
> (pp. 247-8)

This statement about the grammar of *have* in such environments is contentious. A glance at Quirk, Greenbaum, Leech, and Svartvik (1972·80, 388) or Leech (1971:73), will show that the analysis quoted above is inaccurate. Perhaps it should also be said that *Have you a cold/job?* can both sound formal and somewhat stilted in

the face of *Do you have a cold/job?* or *Have you got a cold/job?*

Even if Dulay and Burt's case is unconvincingly presented, subsequent research (their own included) tends to suggest that transfer from NL to TL with respect to very young children may occur with considerably less frequency than with respect to older children and adults.[7] Instead the organisation of the TL as perceived by the learner will provide the hypothesis on which the child's production will be based. Dulay and Burt thus point to the essential weakness in all error analysis - on formal grounds alone it is difficult to give priority to one analysis over another. Dulay and Burt claim superiority for their analysis by placing it within the framework of a psycholinguistic hypothesis which maintains that NL and TL learning are the same processes. Thus their analysis of 'interference-like goofs' is the only one their hypothesis allows. Typically, analyses tend not to rely entirely on formal criteria, since the analyst of TL data brings his own experiential insights to bear on the analysis, resulting in a provisional hierarchy of formal causes of error. An examination of textual data with experiential insights represents a necessary minimum requirement for analysis leading to first order hypotheses as to the source of errors. These hypotheses must still be viewed as tentative until some method is found of refining or discarding them.

Duškova's hypothesis as to the high incidence of the omitted 3rd person singular (-s) ending on English verbs in texts produced by Czech students is a case in point (Duškova, 1969). Here she applies the 'standard analysis', maintaining that the omission is due to overgeneralisation of the rule that assigns no inflections to the other parts of the verb paradigm in the simple present tense. Duškova comments on the fact that the plural morpheme is also frequently omitted and that this cannot be attributed to transfer, since Czech also marks plurals. Nor can there be any question of the students not knowing how to form plurals since they are all capable of writing fairly advanced technical English, and it would be inconceivable for them not to be aware of this grammatical point. Duškova is thus unable to give a linguistically-based explanation but instead proposes that these errors are performance failures or slips of the pen. Why cannot the same explanation be extended to the

other frequent omission error, the 3rd person (-s)? Because, we must conclude, a linguistic explanation is so much neater than a rather vague psycholinguistically based one. Formal explanations are rather more convenient.

4.3 Ignorance

An interesting contribution to the study of the role of the NL in second language learning is contained in Newmark and Reibel (1970). These authors demonstrate how behaviourist theories cannot adequately account for the process of second language learning; consequently, they claim, it is unnecessary to undertake contrastive analyses to determine what errors a learner will make or to attack errors by means of contrastive drills, since the true cause of interference is *ignorance*. *Ignorance* is the state the learner finds himself in every time he tries to express more in the TL than his linguistic knowledge allows him to:

> "The adult can want to say what he does not yet know how to say, and he uses whatever means he has at his disposal *It is easy to see how the phenomenon of interference can result from his attempts to do more than he has yet learned to do in the new language* This seems sufficient explanation of how interference comes about, without the unnecessary hypostatisation of competing linguistic systems, getting in each other's way or taking pot shots at each other"
>
> (Newmark and Reibel, 1970·247, italics added)

James (1971) sees the Newmark-Reibel notion of *ignorance* as merely synonymous with interference. In this he is incorrect, for the terms cannot be compared; additionally he inflates 'ignorance' into a theory, a state of affairs that cannot be justified, since the term 'ignorance' itself is notably lacking in definition and discussion.[8] James maintains that the 'ignorance theory' is not an alternative to interference since they:

> "differ only as to whether using the L1 (NL) is attributable to ignorance of the L2 (TL) or to the influence of the L1 - the argument is otiose ...".

Yet, as we have seen, Newmark and Reibel do not deny the existence of the phenomenon of interference (e g. 1970·245 and the quotation given above). For them, however, the important question is what causes it. Thus, and this is the first critical point, to say that interference is attributable to the influence of the NL is tautological since interference is always defined by reference to a given NL structure.

Secondly, ignorance and interference are not synonyms because, as will be clear from the above quotation from Newmark and Reibel, ignorance is a condition which must logically *precede* interference, since the latter is the product of the former

Thirdly, ignorance need not lead to interference at all - "the learner uses whatever means he has at his disposal" - and as Newmark and Reibel observe:

> "two imperfectly learned languages may infect each other to a greater degree than the native language will infect either one".
>
> (p 248)

Fourthly, James seems unable to decide whether ignorance is ignorance-by-self-evaluation or ignorance-by-observation (i.e as determined on the basis of error analysis)·

> "If extrapolation from the L1 is disastrous, one can say that the learner is 'ignorant' of the L2 form required"
>
> (p 66)

Here James seems to say that the learner is adjudged ignorant-by-observation, on the basis of his performance. He continues:

> "Where extrapolation from the L1 is successful, there is no interference and apparently no ignorance either; yet L2-wise the learner is equally ignorant of the L2 form, whether his performance be successful or calamitous".
>
> (p. 66)

Now James shifts from ignorance-by-observation to ignorance-by-self-evaluation, whereby the learner assesses his capacity to express a particular structure in the TL and finds it

inadequate. This is the fifth difference between *ignorance* and *interference* and James seems implicitly to be acknowledging it in the above. *Ignorance* in the Newmark-Reibel formulation is clearly to be interpreted as *ignorance-by-self-evaluation*:

> "What can (the learner) do other than use what he already knows to make up for what he does not know? ... from the learner's point of view, all he is doing is the best he can: to fill in his gaps of training he refers for help to what he already knows".
>
> (Newmark and Reibel, 1970:247)

As such, *ignorance* may lead to a variety of compensatory solutions. If a linguistic solution is chosen it need not result in error at all, whereas *interference* is by definition an error, and there is nothing in James' use of the term *interference* ("the influence of the L1", "the result of his L1 having led him up a blind alley" - James, 1971:66) which suggests that his is a significantly different interpretation.

We have seen how *ignorance-by-self-evaluation* and *ignorance-by-observation* need not coincide, since the learner may extrapolate successfully from the NL (or apply some other solution) and be judged knowledgeable. There is another aspect that also needs to be considered, namely if the learner believes he knows how to produce the appropriate TL form, but in fact fails to do so. In this case, the learner is not ignorant in his own eyes, even though his knowledge of the TL is faulty.

There are a number of linguistic solutions to *ignorance-by-self-evaluation*. If the learner does not know a particular TL item, he can produce a paraphrase of it (e.g. *The man who takes the rubbish away* for *The dustman* or *It is possible that John will come* for *John may come*) or a transfer from the NL or another language. The solution may be TL-centred in that some part of the TL system known to the learner may serve as a basis (e.g to-complementation requires the verb in the infinitive - *I like to go* **I am used to go*). Another possible solution is more drastic: the learner can change his mind and alter the content of the message as well as its linguistic form (*Don t you think this is a good wine?* - *Yes* (instead of *No, you ve obviously stirred up the*

sediment). Simplification is another solution (*I'll be meeting him later on tonight - I see him tonight*).

Although ignorance-by-observation is interesting and provides much of the material (in the form of errors) on which current research is based, it also presents a potentially false picture of what the learner really does know about the TL. Therefore one should try to uncover what goes on in the learner's mind when he produces text, erroneous or not. One possible way in which one can begin to do this is by elicitation techniques of the kind discussed in Kellerman (1974) (i.e. Chapter One) and Cohen and Robbins (1976), though the methods used are relatively crude still. The fact is that advanced learners are able to verbalise about the text they produce, and valuable insights may be gleaned from what they say, especially when they deal with more complex matters of grammar such as word-formation, syntax and phraseology, where conscious application of strategies may be involved.

4 4 Interference, proficiency, and typological relationships

In continuing his debate about ignorance, James (1971:66) makes the claim that in the case of a hypothetical Italian learning Chinese and Spanish, the Italian's:

> "falling back (i.e using his NL) jeopardises his L2 performance more when it is Chinese than when it is Spanish· this is interference".

Here one assumes that James is relying on the results of a predictive CA, since no empirical evidence is offered in support of his claim. The problem lies with the word *jeopardises*, since it suggests that the Italian learner, speaking a sort of Sino- or Hispano-Italian, will make considerably more interference errors in the former case than in the latter because of the typological differences between Italian and Chinese This statement needs to be examined carefully, for though one can see, as James says, that·

> "acceptable performance [in Spanish is] more easily obtainable than in [Chinese]"
>
> (1971:66)

the point at issue is actually whether there is any positive correlation, as James implies there is, between a high degree of interference and a low degree of *acceptable performance*. In fact, it could be argued that this relationship does not hold, at least in a consideration of such mutually different target languages as Chinese and Spanish, given a NL (Italian) that resembles one and not the other. Instead a high degree of interference should correlate with a high degree of acceptable performance (given that by *acceptable* we mean grammatically correct or communicatively effective), at least in the initial stages of learning Assuming equal opportunities for learning Spanish and Chinese, the Italian learner could hardly fail to become aware of the similarities between Italian and Spanish and the lack of them between Italian and Chinese. When learning Spanish, he will quickly identify cognates, regular relations between the morphological systems of the two languages, syntactic similarities, familiar idioms, etc., so that it is not surprising that interference errors will appear in these circumstances and that certain kinds of error will be resistant to eradication (particularly where NL and TL are minimally different) The Italian could, after all, simply write Italian and yet, as far as a Spanish observer is concerned, be producing 'acceptable performance' (cf. the bilingual poem in di Pietro, 1971:27) Yet the learner of Chinese is faced with a language so different from his own that he will search in vain even for such familiar linguistic 'landmarks' as international words With even grammatical categories in Chinese and Italian often failing to correspond, lack of knowledge of the TL will effectively discourage transfer because the learner is unable to make the cross-linguistic associations and identifications necessary for transfer to take place The outcome is that the errors in Chinese will reflect the structure of Chinese rather than Italian, or show evidence of linguistic simplification

There is some direct and indirect support for the argument that the degree of interference is proportionate to the degree of typological similarity. In published studies of second language performance, Oller and Ziahosseiny (1970) found that speakers whose NL used a Roman alphabet made more spelling errors in English than

those whose NL used another script. Whinnom (1971 96-97), discussing factors which effectively act as barriers to hybridisation (the mixing of one language with another in contact situations) describes what he calls the conceptual barrier as follows:

> "In lexical borrowing, as I suggested in referring to Chinese, there is, below the barrier of phonological compatibility, a still stronger barrier at word level Words, for instance, are not readily transferred to or from a language which has no words in the Indo-European sense, if there is no one-to-one conceptual equivalence, or from a polysyllabic to a monosyllabic language .. I would suggest that the ultimate barrier is conceptual· it is the mode of perception, of reality, which is conditioned primarily by the individual's native language ...".

Whinnom's discussion of *cocoliche*[9] (Whinnom, 1971), while not dealing specifically with the situation of two cognate languages in contact, nevertheless provides support for the above points. Cocoliche was the version of Spanish spoken by Italian immigrants to Argentina to communicate with the native population. There was no stable version of *cocoliche* since the superstrate language, Spanish, was never withdrawn, and the immigrants would speak Italian amongst themselves. Hence Spanish functioned as TL and proficiency in it would range from substandard Italian to *porteño* varieties. This is the situation one would expect whatever the substrate languages, under similar conditions. But, says Whinnom, although Spanish lexical items would have been the first to be assimilated into the Italian morphosyntactic system, this would not occur where Italian and Spanish were sufficiently similar not to hinder communication (e.g. amico-amigo, dovia-debía). In addition, he speculates, alterations in the direction of Spanish syntactic structures might never occur. Furthermore one must assume that *cocoliche* was generally an effective means of communication with no pidgin-like features, and as Whinnom says, had the two communities merged, *cocoliche* "could have become an entirely viable primary language comparable with Middle English, the referential adequacy of its lexicon nowise impaired, its phonological system enriched and its morphology pruned (if at all) only of redundancy" (Whinnom, 1971:100).

According to Todd, much the same processes occurred when the

Vikings and Anglo-Saxons made contact, since:

> "they shared a similar grammar and a considerable lexical stock. Communication common denominators were close at hand and many features common to Germanic languages could be maintained". (Todd, 1974:6)

Presumably the high degree of mutual intelligibility between Italians and Argentinians, Vikings and Anglo-Saxons is not due to any inherent ability on the part of speakers to make wholesale typological comparisons in the way a linguist can try to do. Similarity has to be sensed, and sensed through contact, instruction and learning. It may not take long for the learner to come to the same conclusion as the linguist, i.e. that Italian and Spanish are similar or that English and Finnish are not, but the learner does not have the advantages of the linguist's bird's eye view of the two languages His comparisons are made gradually and incrementally. His problem is to be able to recode the surface structure of the NL so that it appears to be compatible with the surface constraints of the TL systems as he sees them. At the very least this will mean the exchange of lexical items. But even this exchange may cause problems to the learner if he does not 'know' the TL equivalents. He may make good the deficit if he is already aware of formal-semantic equivalences in NL and TL, and a little experience of the TL may be enough to show him that there are a number of cognates in the two languages, give or take a few predictable changes in phonetic and morphological shape. Thus Du *distributie* - E. *distribution*, Du. *inspiratie* - E. *inspiration* are pairs which the 'ignorant' learner can invent on the basis of a little knowledge of the TL. Sometimes the invention will fail, either morphologically, e.g. Du. *compenseren* - **compense*, or semantically, e.g. Du. *instantie* (public body, such as the BBC) = *instance*, *instance* (as in 'This is an instance of interference') = *instantie* (but note that *instance* = *instantie* in *in the first instance* and *in eerste instantie*), or because there is no formal equivalent in one language, e.g. E. *afforestation* = Du. **afforestatie*, or where there is a difference in register, e.g. E. *allocation* = Du *allocatie*, where the latter is rare and extremely formal. A developing sensitivity to NL-TL relationships on the part

of learners may be linked to the commonly observed phenomenon of learners making errors which seem to owe their origin to another foreign language It is not unknown to find English speakers learning, say, a second Romance language using their first Romance language to bear on the second If they know that everybody is tout le monde in French, they might not hesitate to produce todo el mundo in Spanish or tutto il mondo in Italian This process may, of course, also lead to error The gender of French mer (sea) would not be a good guide to the gender of cognate forms in Spanish and Italian (*la mar, *la mare)

Support for the notion that learners are also able to measure up other TLs as sources of transfer to the TL as part of a general ability to compare languages is to be found in a collection of papers edited by Ringbom and Palmberg (1976) If the notion holds good, then very broadly speaking one would predict that reasonably advanced students with native language A, bilingual in language B, and in the process of learning C should more readily make interference errors traceable to A when A is typologically closer to C If B, their second language, is closer to C then there will be fewer errors attributable to A than to B Thus what is important is not what constitutes the first or second language, but its perceived relatedness to a third language This, according to the papers in Ringbom and Palmberg (1976), appears to be the case Their evidence is drawn from the Finnish situation, an ideal one since Finland

> "has two official languages, Finnish and Swedish, which are not related to each other For a long time, there has been no language conflict in Finland The two language groups share a common cultural heritage, and essentially most Swedish-speaking Finns today regard themselves, not as a separate nationality within Finland, but as primarily Finns, with merely a mother tongue different from the majority of the population Thus two groups, linguistically completely different, have an indisputable educational and cultural unity, which would be difficult to find elsewhere"
>
> (Ringbom, 1976 1)

The material is derived from entrance exams to the Abo Akademi, a Swedish-medium university in Finland, which also takes Finnish-speaking students Sjoholm (1976 93) notes that it is often more difficult to identify possible sources of errors made by Finns than by Swedes, adding that

> "it ... seems reasonable to believe that the Finns are aware of the fact that they have little help from their mother tongue in learning English. Instead they appear to rely on generalisations within the target language ... The Finns are also likely to be aware of the similarities between English and Swedish. Therefore comparatively many of the errors among the Finns bear witness of (sic) Swedish influence Only one instance of L3 (i.e. from Finnish) interference could be traced among the Swedes".

The Swedes made relatively many errors attributable to Swedish (p. 98) and almost none attributable to Finnish. Explanations for these results, within the framework being presented in this paper, are not hard to find.

Seen in this light, some of the findings of Schachter, Tyson, and Diffley (1976) can be interpreted. They found that Arab students, while accepting *correctly-formed English* relative clauses as grammatical, tended not to accept as grammatical *Arabic-based erroneous English* relative clauses and *erroneously-formed English* relative clauses *based on other languages*. This finding was in contradiction to their hypothesis that the learner would "identify sentences with his own form of non-native relative clauses as grammatical" and "sentences with other language group forms of non-native relative clauses as ungrammatical" (p. 72). Schachter et al. offer an 'explanation' for this finding, a finding which is at variance with those for other language groups they studied, where responses to erroneous English relative clauses not based on the group's own NL were either to reject them or to treat them randomly. The 'explanation' for the odd figures for Arabs was that four of the ten students were bilingual in French and Arabic, and it was the responses of this subgroup that had skewed the figures with regard to acceptance or rejection of Arabic-like erroneous English relative clauses If the bilinguals' responses were discounted, the remaining responses showed a very strong tendency to accept these latter clauses as grammatical, in accordance with the hypothesis. While the presence of bilinguals in the Arab sample clearly has an effect on the overall picture, this is an observation rather than an explanation. Jordens (1976) explains the bilingual reaction as follows: these learners have already noted the structural differences between Arabic and French (particularly that Arabic

relative clauses have a "pronominal reflex of the deleted noun" while French ones do not - Schachter (1974:208ff), Schachter et al. (1976:71)). Their experience of English shows them that the relatedness of English to French is greater than English to Arabic - hence a suspicion of Arabic-like English structures.

4.5 Projection and the nature of difficulty

The ability to invent new linguistic material on the basis of what is already known is clearly a characteristic of all learners of second languages who attempt to use their language outside the realm of mechanical laboratory drills and exercises. If the learner considers himself ignorant of a specific TL feature, he can use his NL as a make-weight: on the other hand he does not have to be ignorant to do this; he just has to believe that the NL is a viable source for predicting the correct form of the TL. These predictions, as suggested above, are reasoned, being based on accumulated experience of the two languages. The process of extrapolating from the NL to produce a supposed TL structure on the assumption that the two languages are the same I shall call *projection*.[10] Some of these projections will be based on misapprehensions about the relationship between NL and TL and will lead to error. (The examples given above of 'false' cognates will serve to illustrate this point - the learner who misuses *instance* in English can only be doing so because he knows how to use *instantie* in Dutch.)

These beliefs about the NL-TL relationship need not be so strong that the learner considers them to be inviolable. Some will produce errors resistant to eradication (*fossilisations* - Selinker, 1972), others will be 'hunches' that produce transient errors or indicate that the learner is experimenting with alternative forms. Seen in this light the whole notion of *difficulty*, as the term has hitherto been used, has to be revised. For Lado and others difficulty coincides with a measure of linguistic difference, and the correlation between difficulty and error is generally still not questioned:

> "I continue to accept the assumption, which is not usually stated, but which underlies all discussions on this topic, that difficulty of learning is indicated by a greater frequency of error in performance".
>
> (Wilkins, 1972·199)

Yet research into learner's English by means of elicitation procedures and interviews (Kellerman, 1974) suggests that this assumption needs to be reconsidered It is apparent in work of this kind that in a text the occurrence of error need not have coincided with any particular difficulty on the part of the learner, provided that we interpret difficulty in its normal day-to-day sense - which means that difficulty in language learning is a psychological matter. After all, difficulty need not result in error, though the effort expended by the learner in producing a correct form may be considerable. At the same time a linguistically highly erroneous sentence may cause the learner no difficulty at all, because his beliefs have led him to what turns out to be a mistaken projection. He may consider the form he has produced to be a *mastered* form; his belief that what he has produced is the appropriate TL form may only be shaken with effort. Learners will often demonstrate during interviews how firmly committed they are to the belief that they are producing correct English, even rejecting what is in fact a correct form if it differs from their own Here is an extract from an interview:

(A German student had written: *Put in that moment it was six o clock*. She maintained that this sentence was correct English even after being asked to comment on the corrected form with *at* replacing *in*.)

EK: Now you thought you were 100% right. I wrote *At that very moment*.
You are quite convinced that it is *In that very moment*?

S: The only question would be, is it correct to say It <u>was</u> six o clock or It *has been* six o clock. That would be the only question.

(Kellerman, 1974:173-74)

Similarly, a Swiss student rejected forms like *Do you have any time today? Don't you have any cigarettes?* in favour of the more formal *Have you-?/Haven t you-?* structures. She admitted that her English host family used *Do + have*, but since they were of a lower social class, and her teacher in Switzerland had taught her that *do*-less *have* was 'better', she did not use it.

To add to the problem of relating error to difficulty, one of the simplest means available to the learner experiencing difficulty is avoidance or paraphrase of the difficulty (cf Schachter, 1974). In normal language use, the speaker is able to exercise a considerable degree of control over the language he uses Error analysis, as long as it is textually based, is not equipped to deal with these tactics (see Corder, 1973b)

In those cases where errors do occur it will be seen that limiting the perception of difficulty to the learner means that the same linguistic error made by two or more individuals can have quite different statuses within each individual's TL grammar, since what is a mastered form (and therefore 'easy') for one may be another's *slip of the tongue*.

Projection can be a conscious or subconscious process. One hears it being done consciously by learners who use hesitations and question intonations before and during the pronunciation of lexical items or add *Can you say this?* or *Is this the right word?* afterwards. However, it is one side of a two-fold process, as has been hinted above. *Conversion* is different from projection in that it is the application of what is already believed to be known about the TL to the process of projection In other words (and to give some examples) if an English learner of Dutch wishes to express the meaning represented by the English word *capacity* (as in *cubic capacity*) in Dutch, and assuming he did not actually know the word, he might well project (erroneously) from *capacity* to *capaciteit*, since experience of probabilities has told him that this is the sort of relationship between English and Dutch that exists The *projection* is that the meaning symbolised by *capacity* can be expressed by a single word in Dutch, as in English and secondly that this word will be similar, with the appropriate conversions which are regular and predictable Here these *conversion rules* concern

phonetic and morphological changes and are automatic. They are supplementary to the projection and follow it, i.e. the NL form must first be projected and then converted into the appropriate TL form. Conversion rules can also lead to error, as in this attested case from English to French:

English word : creative
French target: créateur
Projection : "cognate form exists" - créatif
Conversion : creat - créat
-ive - -if (cf. progressive - progressif)

(n.b.: some French speakers accept *créatif*)

Conversion is by no means restricted to morphemes. Relexification of NL items is conversion (*I like coffee - j aime le cafe*), but it can operate at higher levels of organisation too. In the sentence **Ik heb zoiets nog nooit in mijn leven gezien* conversion rules are applied to change English word order into the appropriate Dutch order, along with relexifications, agreement, tense, usage etc The projection occurs in the *phraseology* of the sentence, which is derived from English *I have never seen anything like it in my life*. The learner first of all has to decide whether a collocation like *never in (my) life* has a formal equivalent in Dutch (if he decides against it, or does not want to take the risk, he may settle for *never* on its own). If he decides that a similar collocation should exist in Dutch the projection is under way, and a series of conversions are undertaken to change the English surface structure into a Dutch one. The resultant structure remains unidiomatic, however, because the correct Dutch would be *Ik heb zoiets nog nooit van mijn leven gezien* (lit. of my life).

Transfer, when it is the sum of projection plus conversion, is a *strategy* which the learner can apply to the problem of how to express himself in the TL. A strategy is a well-organised approach to a problem, and it is not enough to use the term *strategy* simply to mean *the use of NL in TL*, as has increasingly been the case in recent literature.[11] Instead an attempt must be made to determine how, why and when learners make use of transfer.

4.6 Non-transfer

One other role of the NL which has been given rather less attention in the literature than transfer and interference, probably because of its elusiveness, is its function in the selection of strategies other than transfer. At the same time as learners are learning what can be transferred from NL to TL they are also learning what *cannot*. Again, faced with the problem of having to 'invent' the TL, they may consider an NL feature to be untransferable, because the TL, as they perceive it, would not allow such a feature. My view for the motivation for *non-transfer* owes its origin to several observations: Ferguson (1975) and others have noted that the language used by native speakers to non-native speakers (foreigner talk) shows evidence of formal simplification, presumably for the purpose of ensuring that the 'message' remains uncluttered by redundant elements. In some work I carried out with A. Broeders on Dutch foreigner talk with first year students at Nijmegen University, similar sorts of simplification to those reported by Ferguson were found. While Ferguson's test data did not contain idiomatic sentences for reduction to foreigner talk, ours did, since our intuition was that idioms would be 'simplified'. The standard Dutch sentence

> Ik heb de man over wie u 't heeft niet gezien
> I-have-the-man-about-who-you-it-have-not-seen
> (I didn't see the man you're talking about)

was rewritten by subjects in a number of ways, among which the deletion of the highly idiomatic *t over ... hebben* invariably took place, and was either not replaced, e.g.

> de man - U - niet gezien
> (the man - you - not seen)

or replaced by more 'basic' words such as *bedoelen* (mean), *spreken* (speak), *praten* (talk), *zeggen* (say). This sort of finding is also reported by Henzl (1973·220), who writes:

> "...the fact that idiomatic expressions are commonly paraphrased or substituted in samples of speech directed to foreign students

> (by native speakers) implies that the speakers actively control their vocabulary. However ... I am reluctant to draw the conclusion that a native speaker is fully aware of the intrinsic idioms of his speech".

Even if a native speaker is probably not 'fully aware of the intrinsic idioms' in his NL, observation of the spoken and written language of learners does suggest that, *inter alia*, idioms are one class of language items that are generally not transferred, even when it would be quite possible to do so and produce correct TL. I have often noted the amazement on our students' faces when they do discover the existence of Dutch-like idioms in English. And this reaction is not restricted to idiom, but includes ordinary lexical items as well, since learners will sometimes not transfer or accept the most 'innocent' items. A case in point is the word *voetballer* in Dutch. This is normally translated by *footballer* in English and *Fussballer* or *Fussballspieler* in German. But Dutch students almost invariably translate the word as *football player* and *Fussballspieler*.

Idioms, then, are only part of a potentially large class of items which a learner may at any given moment treat as *language-specific* (L-S). A language-specific item in this sense is a NL feature which a given learner tends not to transfer to a given TL. Such features can be contrasted with *language-neutral* (L-N) items which the learner believes can be transferred to a given TL. The role of the TL in the assignment of specificity or neutrality is important here because the perceived relationship between NL and TL will affect the learner's judgements. Thus the specificity of an item is relative and not generally intrinsic. The outcome of this position is that a learner may transfer an item to one TL but not to another.

An approach to transfer strategies that appears to take a point of view similar to the one given here is outlined by Schachter (1974:212), who discusses the absence of certain syntactic structures in the TL production of learners with given NLs:

> "The learner apparently constructs hypotheses about the target language based on the knowledge he already has about his own language. If the constructions are similar in the learner's mind, he will transfer his native language strategy to the

target language If they are radically different, he will either reject the new construction or use it only with extreme caution".

I do not believe that the learner has to form specific hypotheses about constructions being similar or different on the basis of actual contact with those constructions If the learner predicts that the construction will be the same as in his NL he will transfer. if he predicts that it will be different he will not The supposition is that contact will also affect behaviour, and it is possible that those students showing avoiding tendencies after the contact phase (i e they are aware of what the TL construction is actually like) may not have done so in the projection phase It will be seen that my position leads to predictions about learner behaviour different from those made by means of orthodox CA These differences are summed up in fig 1

Orthodox CA		
NL linguistic unit	difference between NL unit and TL equivalent	result
X	nil	no difficulty or error
	small	some difficulty and interference
	great	considerable difficulty and interference

Language-specific/language-neutral hypothesis				
Learner's NL	L-S/L-N assignment	projection?	actual NL-TL difference	result
X	L-S	no	irrelevant	omission, para-phrase, etc
(may not co-incide with linguist's unit)	L-N	yes	nil	no error
			small	error
			great	error

Fig. 1: Differences in prediction of learner behaviour between the CAH and the L-S/L-N hypothesis

4.7 Preliminary investigations into the existence of the transfer strategy

What follows below is an account of some informal experiments designed to explore the substance of the *Language-specific/Language-neutral* distinction as it relates to judgements of acceptability. It was decided to examine whether students of English at the University of Nijmegen would treat a class of NL items strategically, assigning them to either the L-S or the L-N category. A number of Dutch idiomatic expressions were chosen for investigation for reasons given above. Subjects drawn from advanced learners of English at a college of education and a university were given a list of sentences, ostensibly in English, and were asked to judge whether they were correct or not. They were told that some sentences were completely correct but not the proportion of correct to incorrect The sentences could be analysed in four separate ways, namely

a. those sentences containing a Dutch-like idiomatic expression which was also correct in English (+Du+E+id), e.g. *to have the victory in the bag* (see Appendix)
b. those sentences containing a Dutch-like idiomatic expression which could not be transferred to English (+Du-E+id), e.g. *to be the cigar* (see Appendix)
c. sentences containing idioms not possible in Dutch, e.g. *it s no use crying over spilt milk*
d. sentences containing idioms not possible in either Dutch or English.

In addition there were an equal number of filler sentences not containing idioms but designed to test reactions of a similar kind to those contained in the above sentences. These will not be discussed further, nor will sentence types c. and d. above. What follows, then, is a discussion of sentence types a. and b

Each student was given seventy sentences of the kind outlined above (that is to say thirty-five idiomatic ones and thirty-five non-idiomatic ones) of which ten were *+Du+E+id* and ten were

+Du-E+id. Students were asked to underline those parts of each sentence which they felt were wrong and only those parts If a student underlined a complete idiomatic expression, inter alia, it was assumed that he found the expression unacceptable in English and that he would not transfer it. If he did not underline an idiomatic expression it was assumed that he either knew that the expression was possible in English or that it might be. Since there were ten sentences of each kind (i.e. +Du+E, +Du-E), a student with perfect knowledge of the correctness of those expressions would score no rejections of those sentences marked +Du+E and ten rejections of those marked +Du-E. Thus *perfect knowledge* would be indicated by a score of the difference between these two scores, that is, 10 If, on the other hand, students rejected all the sentences, they would appear to be treating idioms according to a strategy based on an assumption of *language-specificity*, the difference being zero If they accepted all twenty sentences they would appear to be using a strategy based on an assumption of *language-neutrality*, also scoring zero. These two strategies could be distinguished from each other by the tendency to reject or accept. If the tendency was to accept, then the student treated these idioms as transferable If he tended to reject them, then the student treated them as non-transferable

There were two hypotheses to be tested. These were

I. College of education and first-year university students would tend to reject Dutch-like idiomatic expressions, whether correct in English or not, to a greater degree than more senior years.
II. Third-year university students would tend to distinguish between Dutch-like idioms which were correct in English and those which were incorrect, i e display 'knowledge'.

The reasoning behind hypothesis I is that College of education and first-year students are relatively naive, linguistically speaking, and that the emphasis in schools is on communication rather than grammatically perfect production. The result is that idiom teaching plays a relatively small part in the school curriculum, and students will be forced to rely on their own 'feel' for the languages concerned.[12] In view of the behaviour of linguistically naive native

speakers addressing foreigners in a simplified register (Ferguson, 1975) and the anecdotal evidence for non-transfer given above, it seems plausible that students at less proficient levels will generally reject Dutch-like idioms. The reasoning behind hypothesis II is simply that students learn a great deal in three years, idioms included, of course.

The results of the experiment can be shown in tabulated form (Table 1):

	RT	ST	SD	SDE	K
CE	79	57	86	29	15
O	67	65	81	48	29
1	63	66	78	53	31
2	66	70	85	54	39
3	60	77	80	74	54

(all figures are percentages, rounded off to whole numbers)

CE: College of Education students, first year (N = 24)
O : university students, first year, tested on entry 1976/77 (N =24)
1 : university students, first year, tested at end of 1975/76 (N = 24)
2 : university students, second year (N = 24)
3 : university students, third year (N =24)
RT: rejection scores for all twenty expressions
ST: successful decisions for all twenty expressions
SD: successful decisions for the ten expressions incorrect in English
SDE: successful decisions for the ten expressions correct in English
K : 'knowledge' scores (see text)

Table 1: Results of idiom experiment

It will be seen that there is generally a clear progression in the various scores. Third-year students made the fewest rejections, and CE students (who have had a lower level of secondary education and receive less tuition in English than university students) the

most. Similarly, third-year students made the most correct decisions and CE students the least. However, there is very little difference in the 'success' scores for the incorrect expressions across the groups. This suggests that third-year students are no better than the other groups at recognizing incorrect expressions. In fact they tend to be slightly (but not significantly) worse at this task than all but first years 1975/76. They are clearly better at spotting the correct expressions than the other groups.

K-scores are a relative measure of the 'knowledge' in each group. For an individual to have native-speaker-like knowledge of the correctness of these expressions in English, he would have to reject all the incorrect expressions and none of the correct ones. Since there were ten expressions of each kind, this means that perfect knowledge could be represented by the difference between the rejection scores, i.e. 10 (10-0). A single approach to all twenty expressions not based on knowledge would lead either to twenty rejections (10, 10) or no rejections (0, 0). In both cases the difference ('knowledge') is zero. Thus the scores ranging between 10 and 0 is a 'knowledge'-'other principle' scale, with 5 being indeterminate. Simply counting success scores does not provide this kind of measure, since it will be impossible to distinguish between a person with, say, five rejections of each type and one who rejects all twenty. In both cases the success rate is 50%. Thus in Table 1 we can see, using the measure of differences, that only 15% of CE students' decisions could be due to knowledge, as against 54% for third-year students.

T-tests on the group scores for the ten <u>incorrect</u> expressions reveal no significant differences between groups. As to the differences in scores for the ten correct expressions, t-tests show that some differences are significant.

	CE			
0	t = 3.49 p < .01			
		0		
1	t = 4.25 p < .01	t = 1.33 n.s.		
			1	
2	t = 4.74 p < .01	t = 1.06 n.s.	t = 0.20 n.s.	
				2
3	t = 9.27 p < .01	t = 4.95 p < .01	t = 3.37 p < .01	t = 3.42 p < .01

Table 2: Results of t-tests on group differences in treatment of correct expressions

These figures tend to confirm the apparent shift from 'strongly strategic' to 'knowledgeable' across the years, with most students treating the expressions language-specifically. The very few students who tended to accept all the expressions were incidentally noted as being among the 'weakest' generally.

Thus, hypothesis I, that first-year English students would tend to reject Dutch-like idioms was confirmed. Jordens (1976) predicted that, as German and Dutch were typologically close, incoming students would accept Dutch-like idioms irrespective of their correctness in German. This was indeed the case, there being a very strong tendency to treat them language-neutrally. Surprisingly, however, second-year German students treated the same expressions language-specifically. These findings could be explained in terms of the transfer strategy discussed in this paper. I said that an item was adjudged language-specific if the learner did not believe it could be transferred to a given TL. Dutch and German are closer in many respects than Dutch and English, and students are probably unaware of many of the differences in the first pair, especially at the phraseological level, until a comparatively later stage than is the case with the second pair.

Hypothesis II, that third-year students would be more successful at distinguishing correct English idioms similar to Dutch ones from Dutch-based erroneous idioms was confirmed. In examining the rejection scores of the third-year group, I noticed a tendency (admittedly slight) to be more generous towards erroneous Dutch-based expressions. I think this is probably a by-product of their increasing awareness of the similarities that do exist between the two languages, and a resultant tolerance of such expressions Additionally, this means that this group was no better at rejecting erroneous sentences than the other groups; that is to say that at least as far as idioms are concerned, their knowledge of what is possible in English has outstripped their knowledge of what is not possible, and that little progress is made in this respect in the three years.

The connection between a general tendency to reject or accept an idiom may, of course, be connected with the linguistic form or semantic transparency of the idiom, and indeed there does seem to be a correlation between opacity and rejection. Thus the idiom in the sentence *Do you remember what we were having it over? (talking about)* which is semantically opaque to the uninitiated was almost totally rejected by all subjects, while *I don t think he should have insulted her behind her back* was generally accepted (though over a third of the first-year groups rejected it) Yet the expression *to have the victory in the bag* (+Du+E) was heavily rejected by all but third-years, while first-year groups heavily rejected *to lay it on thick* (+Du+E). Another opaque idiom, *dyed-in-the-wool* (+Du+E) was rejected by only half the students. Thus although from this limited evidence one can say that opacity and language-specificity are related, the relationship is not total

Another avenue to explore has been opened up in a publication by van Lancker (1975:145) in the field of neurolinguistics Briefly, van Lancker distinguishes between two modes of speech, propositional and automatic, which are differently processed in the brain. Evidence for this distinction comes from observation of aphasics, amongst other things.

> "Aphasics often lose abilities for propositional speech, but variously retain .. utterances such as swearing, counting,

> stereotyped phrases, conventional social formulas, familiar phrases, highly frequent items, idioms and stock expressions".
>
> (italics added)

There is evidence, apparently, that these types of automatic speech have quite different properties from propositional types Van Lancker goes on to review the way a number of writers have dealt with the problem of idiom in languages and suggests that idioms, in themselves gradable, exist on an automatic-propositional continuum somewhere on the automatic side of conventional propositional speech If they are, as a rule, neurologically distinguishable from ordinary speech, then here we may have a biological basis as well as a linguistic one for the particular strategies that learners apply to idiomatic expressions

4.8 Conclusions

This chapter argues for the existence of transfer within a cognitive framework. Transfer is to be seen as an option open to learners either when faced with the need to compensate for a perceived knowledge deficit in the TL or because they believe that the NL is a viable source of prediction as to the nature of the TL. In contradistinction to the CAH, this view of transfer does not equate linguistic difference with interference. On the contrary, it would claim that transfer is more likely to occur where NL and TL are typologically closer than where they are distinct Thus perceived language distance is one constraint on the process of transfer.

A second constraint on transfer depends on the particular class of NL items available for transfer. Some items, it is claimed, are resistant to transfer even between close languages. Idiomatic expressions form one such class of items. The experimentation conducted here shows that in areas where learners have little or no real TL knowledge, strategies are developed to deal with the problem of TL production The hypothesis was that idioms would be dealt with strategically, the strategy would be not to judge Dutch-like expressions as good as English, irrespective of whether they were possible in English or not. The hypothesis was confirmed Even advanced learners of English treat Dutch-like idioms as

untransferable. A similar experiment with Dutch learners of German showed the importance of language distance and cumulative experience of the TL. Less proficient learners generally allowed the Dutch-like expressions in German, while more advanced learners rejected them. These experiments thus support the notion of the existence of a strategy of transfer available to learners under the appropriate conditions. Neither the CAH nor ignorance can, it is felt, account as effectively for certain 'errors' of commission or omission as the view of transfer propounded here

NOTES TO CHAPTER FOUR

1. This chapter is a revised version of the article which appeared in *Interlanguage Studies Bulletin*, 2, 1977, pp. 59-145.

2 The terms *formal cause* and *efficient cause* are taken from Aristotle Their use was suggested to me by David Reibel.

3 There is a good deal of evidence to the contrary. See e g Selinker, Swain and Dumas, 1975, Kinzel, 1964 Even Dulay and Burt retreat a little from this adamant 'no-interference' position in their 1973 paper.

4. There are a number of problems in the way Dulay and Burt handle their case. Two weaknesses are admitted·

> "What can we say about those (TL) structures which have no corresponding (NL) data analysis for comparison? Nothing except suggest the task for future research",
> "There is evidence from interference structures, e g. Ravem's finding in *yes-no* question formation by a Norwegian child learning English To account for this one might make the weak argument that because of the limitations of natural data collection, utterances reflecting, say, subject-verb inversion in *yes-no* questions might have been made by Adam, Eve and Sarah when Brown and his colleagues were not there to collect them" (pp. 243-44).

5. Even if it were found that such forms appeared irrespective of the child's NL, there would still be no proof that identical surface representations could not have quite different sources. One learner's transfer could be another's faulty generalisation of TL material.

6. Ravem (1968)

7. It is also claimed by e.g. Sciarone (1970) and Taylor (1975) that as adults become more proficient, so interference decreases and the so-called generalisation error increases.

8. The term *ignorance* appears once in Newmark and Reibel (1970).

9 Whinnom's information about cocoliche is derived from Entwistle (1936·274-5)

10. Projection is confined to transfer and interference only for the purposes of this discussion; naturally it concerns any utterance based on systems already known, whether belonging to the TL (*overgeneralisation, false analogy*, etc.), or another language (L_3 interference).

11. Taylor (1975) is one who talks of *transfer learning strategies* when he really means *interference* and *overgeneralisation strategies*. It is difficult to see how a learner can employ a strategy designed to *over-generalise* - while Schachter (1974) gives the term an even more obscure twist in referring to a) the errors resulting from 'the strategies employed by the learner in the acquisition of the target language' (p. 206) and b) 'the ... restrictive relative clause formation strategies of four ... languages' (p. 207).

12. Irujo (1984) showed that none of the following variables had any positive effect on the knowledge of English idioms amongst learners: years of formal study, parents' knowledge of English, years in school, age of arrival in USA, length of residence in USA, liberal arts education, knowledge of other foreign languages. [Note not in original text]

APPENDIX

The 20 sentences containing Dutch-like idiomatic expressions

A. Correct English

1. Brian Graham is a dyed-in-the-wool conservative.
2. We should take the bull by the horns and buy the property, even if it is risky
3. He's normally a very good actor in tragedies, but tonight he really laid it on thick, don't you think?
4. Everything she tells her son goes in one ear and out the other
5. I wouldn't like to be in his shoes when his boss finds out he stole the money
6. If those are going to be your arguments, you won't have a leg to stand on
7. I don't think he should have insulted her behind her back
8. This is precisely the point! You've hit the nail on the head
9. It was, the prosecution lawyer claimed, murder in cold blood.
10. With a lead of 4 - 1, Ajax had the victory in the bag

B. Incorrect English (with possible corrections)

11. Despite the fact that we were all guilty it was me who was the cigar. ('copped it')
12. We all know Rupert misbehaved, but why don't we just look at it through the fingers this time. ('turn a blind eye')
13. Do you remember what we were having it over last week? ('talking about')
14. Even after they splashed water on his face, he still didn't come by. ('come round/to')
15. The man's worried expression made me realise there was something serious on the hand ('going on')
16. On seeing the police car, he quickly took his legs and disappeared ('ran off, took to his heels')
17. You're going to marry Jacqueline Onassis? Oh, for heaven's

sake make it a little'. ('come off it')

18. You know what they say: "Rest rusts". ('A rolling stone gathers no moss'(?))
19. Linda's no fool. You couldn't sell her turnips for lemons. ('pull the wool over her eyes')
20. I like him most of the time, but his behaviour last night really spanned the crown. ('took the biscuit')

5. PREDICTING TRANSFERABILITY FROM SEMANTIC SPACE[1]

5.0 Introduction

In Kellerman (1977) (i.e. Chapter Four) an attempt was made to characterise a *strategy of transfer* in second language learning and performance, whereby a learner with a given native language (NL) could use that language to make predictions about the target language (TL), transferring NL forms and features whenever it was felt that they could be successfully employed in the TL (*projection*), with suitable adjustment being made according to the supposed constraints imposed by TL surface structure (*conversion*).

The learner could *project* a) so as to fill a perceived gap in his knowledge of the TL, or b) because he believed that NL and TL are to all intents and purposes identical either in very specific detail or in more general terms. Much will presumably depend on the learner's notion of the *distance* between NL and TL: the closer the TL is felt to be to the NL, the more useful a strategy of transfer is likely to be.

In the same paper, it was pointed out that if the learner did not believe that a particular NL form or feature could have a parallel existence in the TL, he would not, in the normal run of things, transfer. Thus for a given learner with a given TL, it would be theoretically possible at a given moment to list those items in his NL that he considered *language-specific* and thus not transferable to the given TL, and those he considered *language-neutral*, i.e. transferable to the given TL. The TL itself is important here, since NL items should not necessarily be seen as inherently either transferable or non-transferable. A NL item i, for instance, may be labelled *specific* in terms of TL_a, but *neutral* towards TL_b. Additionally, with the increase in 'real' knowledge of the TL on the part of the learner (i.e. when both learner and some external authority agree independently that a given language sample he produces is the TL), the assignment of these labels to NL items will be under constant revision.

The paper then goes on to describe an experiment designed to test how Dutch learners of English at university level would react

to picturesque Dutch idioms if they were translated into English. What happened was that students tended to reject such translations as incorrect English, irrespective of whether these idioms existed in English or not. Third-year students were better at spotting which idioms were correct in both English and Dutch ('real' knowledge) but also tended to be slightly more generous to those which were not, suggesting that, while they may have learnt a great deal about what is possible in English, they still had something to learn about what is not. First-year students tended to reject all idioms if they were Dutch-like. Thus, on the evidence of these experiments (plus much that is anectodal) this kind of idiom is characteristically treated as language-specific, a state of affairs that only the acquisition of 'real' knowledge will alter.

5.1 Transferability

The rest of this paper will be devoted to testing the notion of transferability in lexis. Transferability is a measure of the relative specificity of a NL item in terms of comparable items. It is independent of perceptions of language distance, though it will interact with these to partially determine actual performance. Transferability is thus a theoretical construct which makes predictions of the following kind: *if item i is more transferable than item j, then if an ideal population of learners consistently transfer j, they will also transfer i, though the converse is not necessarily true. If j is not transferred, then no prediction can be made about i.* The relationship is thus implicational. In terms of the idiom experiment, idioms appear to have low transferability.

The fact that idiom translations are not seen as feasible between two such close languages as Dutch and English indicates that learners are sensitive to the special nature of such lexical phenomena. If such intuitions about the specificity of these expressions were to be found exclusively amongst advanced learners, we might say that teaching methods, with their accent on language differences, were responsible. But it seems rather more generally true, irrespective of the level of proficiency of the Dutch learner of English at least: true for university students, as well as for doctors, nurses and other professional people brushing up their

English at evening classes, and, as we shall see, even true for quite young schoolchildren.

The reactions to what appear to be gross translations of Dutch idioms into English are interesting, in that they can provoke quite marked responses. Such apparent calques are stigmatised as 'silly', 'ridiculous', 'too Dutch', 'impossible in English', or are greeted with sniggers. The strength of the reaction can perhaps be gauged from the following dialogue which is based on an actual classroom incident:

Teacher: "'Out + verb' is very productive in English, not like Dutch, where you can only say *Ik liep hem eruit* ('I ran him out of it') which not everybody agrees is acceptable Dutch. You can outrun, outjump, outthink, outplay, outdrink, you know, outdrink someone - you can drink him under the table ..."

Student at evening class (brushing up her school English for professional purposes): "Excuse me, but what is the correct English for that expression?"

Teacher: "Sorry?"

Student: "The correct English for the Dutch expression..."

Teacher: "What Dutch expression?"

Student: "*Iemand onder de tafel drinken.*"

Teacher: "Is that Dutch too?"

The mutual mystification evident from the above seems to indicate that learners assign special status to idioms An idiom is very often semantically intractable to non-natives and may reveal syntactic idiosyncracies as well It may have special neurological status too (see van Lancker, 1975). It would be convenient to say that, psychologically speaking, idioms are 'marked'[2] structures in one's native language. From here it is not difficult to follow a

line of reasoning that would assume that, if the form of idioms were already 'special' in one's own language, the likelihood of finding parallels in the language one is learning would be remote. Hence the more 'marked' an item, the less transferable it should be [3]

So far the discussion has limited itself to idiomatic expressions and such notions as language-distance, specificity, neutrality, transferability and 'markedness'. A more rigorous examination of the relationship between intuitions about 'markedness' and transferability would involve more extensive and perhaps more homogeneous material. With such material it might be possible to establish a 'markedness' *gradient* which would be of greater interest than the almost uniformly highly marked idioms [4] The 'markedness' gradient could then be used to predict the differential transferability of the items If we may now formulate a working hypothesis, *there should be a strong correlation between the degree of 'markedness' of an item and its relative transferability.*

A lexical item whose meaning varies according to linguistic context is ideal material for such an investigation. Such a 'word' may cover intuitively quite distinct meanings, or metaphorical extensions of a basic concrete sense, or senses with more subtle shades of meaning. A 'word' that fills the bill adequately is the verb *break* and its Dutch counterpart *breken*. This is not the place to go into discussion about homonymy and polysemy, but the sorts of senses subsumed under *break* in English are extensive as any dictionary will show. Furthermore many of the meanings of *break* and *breken* overlap.

It is possible, certainly on intuitive grounds, to select the meaning enshrined in *He broke his leg* or *Hij brak zijn been* as the primary meaning of *break* or *breken* 'of a solid· [cause to] separate into two or more parts as a result of e g a blow'. This is more or less the definition that native speakers do produce first if asked to define *break* or *breken*. For them not to do so would be perverse. In the same way, a request for a definition of a word like *blue* should yield a reference to *colour* before *depression, jazz* or *dirty jokes or films*. The *primary* meaning is thus the 'unmarked' meaning, and its 'primariness' may be a composite of high frequency

of occurrence, syntactic flexibility, literalness or concreteness[5], etc. It would be more difficult to assign degrees of 'markedness' to non-primary items on a priori (e.g. linguistic) grounds. An immediate problem arises as to the status of metaphorical extensions. Thus it is difficult to relate *breaking hearts* and *breaking voices* to the primary meaning and to each other according to some simple attribute. The grading of the various senses clearly exists in more than one dimension.

The experiments about to be described divide into three sections:

a) Gathering data about transferability from Dutch learners of English
b) Gathering data about the intuitions of native speakers of Dutch
c) Establishing correlations between the two sets of data

5.2 Gathering transferability data

The first part of the experiment consists of two phases. Phase one was carried out with 210 Dutch subjects (students and schoolchildren) All subjects were learning English; the 109 students were studying English full-time at Nijmegen University. The sample divides up into the following groups of subjects:

University

Second-year students, tested at end of academic year (NU2), n = 26

First year students, 1976-1977, tested at end of academic year (NU1), n = 50

First-year students, 1977-1978, tested at beginning of academic year (NUO), n = 33

Secondary school

Sixth-year pupils (A6), n = 17
Fifth-year pupils (A5), n = 23
Fourth-year pupils (A4), n = 18
Third-year pupils (A3), n = 20
Second-year pupils (A2), n = 23

Subjects were given nine Dutch sentences containing a sense of *breken*. The instructions were simple. If subjects thought the sense of *breken* illustrated by a particular sentence could be translated into English by *break*, they were to mark the sentence with a cross Otherwise they were to do nothing. The nine sentences (with the English word in italics serving as mnemonic for the Dutch sense) were as follows·

1. De golven braken op de rotsen (The *waves* broke on the rocks)
2. Zijn stem brak toen hij 13 was (His *voice* broke when he was 13)
3. Het kopje brak (The *cup* broke)
4. Zijn val werd door een boom gebroken (His *fall* was broken by a tree)
5. Hij brak zijn woord (He broke his *word*)
6. Na het ongeluk is hij een gebroken man geworden (After the accident, he was a broken *man*)
7. Hij brak zijn been (He broke his *leg*)
8. Zij brak het wereldrecord (She broke the world *record*)
9. Zij brak zijn hart (She broke his *heart*)

Every acceptance of *breken* = *break* was counted for each item, yielding the following group scores·

	Group									
	NU2	NU1	NUO	A6	A5	A4	A3	A2	Totals	mnemonic
1	54	32	18	12	4	11	25	39	26	
	(14)	(16)	(6)	(2)	(1)	(2)	(5)	(9)	(55)	'waves'
2	54	40	12	0	0	6	20	9	21	
	(14)	(20)	(4)	(0)	(0)	(1)	(4)	(2)	(45)	'voice'
3	81	64	76	82	100	100	100	83	82	
	(21)	(32)	(25)	(14)	(23)	(18)	(20)	(19)	(172)	'cup'
4	19	32	6	6	13	22	20	48	22	
	(5)	(16)	(2)	(1)	(3)	(4)	(4)	(11)	(46)	'fall'
5	88	70	88	82	26	56	45	48	66	
	(23)	(35)	(29)	(14)	(6)	(10)	(9)	(11)	(139)	'word'
6	81	80	64	65	48	72	70	52	68	
	(21)	(40)	(21)	(11)	(11)	(13)	(14)	(12)	(143)	'man'
7	96	100	97	100	96	100	100	83	97	
	(25)	(50)	(32)	(17)	(22)	(18)	(20)	(19)	(203)	'leg'
8	65	52	45	59	0	44	20	57	45	
	(17)	(26)	(15)	(10)	(0)	(8)	(4)	(13)	(94)	'record'
9	88	98	97	94	74	94	90	70	89.5	
	(23)	(49)	(32)	(16)	(17)	(17)	(18)	(16)	(188)	'heart'

Table 1: Acceptance scores for *breken = break* expressed as percentages. Raw scores in brackets.

These figures give us a series of rank orders per group, ranging from 'most acceptable' to 'least acceptable' sentences (Table 2).

	Group							
	NU2	NU1	NUO	A6	A5	A4	A3	A2
1	leg	leg	leg	leg	cup	leg	leg	leg
2	heart	heart	heart	heart	leg	cup	cup	cup
3	word	man	word	cup	heart	heart	heart	heart
4	cup	word	cup	word	man	man	man	record
5	man	cup	man	man	word	word	word	man
6	record	record	record	record	fall	record	waves	word
7	voice	voice	waves	waves	waves	fall	record	fall
8	waves	waves	voice	fall	record	waves	fall	waves
9	fall	fall	fall	voice	voice	voice	voice	voice

Table 2: Rank orders of acceptance scores by group

The second phase is essentially the same, except that the number of *brekens* is increased from 9 to 17 This brings with it obvious benefits for testing the hypothesis, and will bear particularly on the collection and processing of native speaker intuitions, as we shall see.

Subjects (n = 81) were drawn randomly from Dutch first and third-year students of English at Utrecht University The first-year group (UT1) consisted of 50 subjects, the third-year group (UT3) of 31. The following 17 sentences were used (with the original 9 being marked by an asterisk)

*1 De golven braken op de rotsen (The waves broke on the rocks)

2. De lichtstralen breken in het water (The light rays refract in water)

*3 Hij brak zijn been (He broke his leg)

*4 Het kopje brak (The cup broke)

*5. Na het ongeluk is hij een gebroken man geworden (After the accident, he was a broken man)

*6 Hij brak zijn woord (He broke his word)

*7. Zij brak zijn hart (She broke his heart)

8 De man brak zijn eed (The man broke his oath)

9. Welk land heeft de wapenstilstand gebroken? (Which country has broken the ceasefire?)

10 Sommige arbeiders hebben de staking gebroken (Some workers have broken the strike)

11 Nood breekt wet ('Necessity breaks law')

12. Dankzij een paar grapjes was het ijs eindelijk gebroken (Thanks to a few jokes the ice was finally broken)
13. Een spelletje zou de middag enigszins breken (A game would break up the afternoon a bit)
*14. Zij brak het wereldrecord (She broke the world record)
*15. Zijn stem brak toen hij 13 was (His voice broke when he was 13)
*16. Zijn val werd door een boom gebroken (His fall was broken by a tree)
17. Het ondergrondse verzet werd gebroken (The underground resistance was broken)

Of the additional sentences, nos. 2 and 11 have no direct English equivalents. The latter, a Dutch proverb, is odd in that the normal word used for breaking the law is not breken but overtreden (infringe).

The group acceptance figures, expressed in percentages of total possible acceptances, are as follows (raw scores in brackets):

	UT3	UT1	Totals	mnemonic
1	55 (17)	36 (17)	43 (35)	'waves'
2	29 (9)	16 (8)	21 (17)	'voice'
3	74 (23)	82 (41)	79 (64)	'cup'
4	19 (6)	22 (11)	21 (17)	'fall'
5	68 (21)	78 (39)	74 (60)	'word'
6	77 (24)	74 (37)	75 (61)	'man'
7	100 (31)	100 (51)	100 (81)	'leg'
8	71 (22)	58 (29)	63 (51)	'record'
9	100 (31)	96 (48)	97.5 (79)	'heart'
10	32 (10)	30 (15)	31 (25)	'light rays'
11	55 (17)	60 (30)	58 (47)	'oath'
12	35 (11)	34 (17)	35 (28)	'ceasefire'
13	6 (2)	14 (7)	11 (9)	'strike'
14	42 (13)	42 (21)	42 (34)	'law'
15	55 (17)	32 (16)	41 (33)	'ice'
16	13 (4)	14 (7)	14 (11)	'game'
17	16 (5)	34 (17)	27 (22)	'resistance'

Table 3: Acceptance scores, Utrecht, for breken = break expressed as percentages Raw scores in brackets.

These figures give the following rank orders, ranging from 'most acceptable' to 'least acceptable':

	UT3	UT1	Overall
1.	leg*	leg*	leg*
2.	heart*	heart*	heart*
3.	man*	cup*	cup*
4.	cup*	word*	man*
5.	record*	man*	word*
6.	word*	oath	record*
7.	oath	record*	oath
8.	waves*	law	waves*
9.	ice	waves*	law
10.	law	ceasefire	ice
11.	ceasefire	resistance	ceasefire
12.	light rays	ice	light rays
13.	voice*	light rays	resistance
14.	fall*	fall*	fall*
15.	resistance	voice*	voice*
16.	game	game	game
17.	strike	strike	strike

Table 4: Rank orders of acceptance scores, Utrecht (items also appearing in previous experiment marked with an *)

The rank order correlation between the two Utrecht groups is high (Spearman's rho = .919, significant at <.01).

5.3 Comparison of results of the transferability experiments

If we compare the ranking of the nine original items across all ten groups used in the experiments, it will be clear that the Utrecht sample is not substantially different in its behaviour from the earlier sample. Kendall's coefficient of concordance, W, based on the rank orders of the nine original items for the ten groups, is .9047, significant at <.01. Thus the rank orders are very closely related to each other.

What is therefore noteworthy is this consistency between groups, despite the range of ages and experience in the sample. The conclusion one must reach is that the ten groups are drawn from essentially the same population, qualitatively speaking, and that the effects of teaching, learning and growing older do not significantly alter learner's beliefs about the relative transferability of the *breken*s. Clearly we are dealing with an implicational series of items of considerable generality. The overall order for all 291 subjects is:

1. leg
2. heart
3. cup
4. man
5. word
6. record
7. waves
8. fall
9. voice

Table 5: Overall rank order of transferability for ten groups

One interesting feature in the results from the two samples deserves comment. It is evident that there is a certain discrepancy in the scores relating to *leg* and *cup* in groups NU1, NUO, UT3 and UT1, and to some extent in A6 and NU2. This discrepancy is not noted in the scores of the other groups. Thus the university group (including A6) tends to find *breken = break* more acceptable for *Hij brak zijn been* than for *Het kopje brak*, the difference in treatment of the two items being statistically significant ($t = 7.142$, $< .01$). A possible explanation for this phenomenon resides in the difference between causative and non-causative *breken/break*, with the former being adjudged 'unmarked' relative to the latter. This point is discussed at some length in Kellerman (1979a,c)[6]

5.4 Gathering native speaker intuitions

How native speakers perceive the inter-relatedness of the meanings of *break* or *breken* will be crucial for assigning the appropriate degree of 'markedness' to each sense. From these native

speaker intuitions, it will be possible to construct a semantic space (see e g Clark and Clark, 1977) from which the dimensions along with judgements of inter-relatedness are made may be revealed

To arrive at a representation of such a semantic space, a sample of native speakers is required to make judgements about the similarity of the senses to each other These judgements can take various forms, but a point in common is that all such judgements should be convertible to numerical values The goal is to arrive at a similarity matrix, where judgements of similarity are converted to distance scores All this means is that subjects may be asked to rate similarity according to a given scale, say 1-5, where '1' could mean *identical in meaning* and '5' *totally unrelated in meaning*. Thus for any pair of meanings, similarity can be expressed as the sum of the ratings for that pair in a given sample

In the experiments reported here, this method of paired comparisons was not used, as pre-testing had shown it to be difficult, unreliable and tiring where polysemy was concerned Instead, use was made of Miller's card sorting method for gathering similarity data (Miller, 1969) This method was developed by Miller to investigate the structure of the mental lexicon

In his study, subjects were asked to sort 48 nouns typed separately on cards into piles according to *similarity of meaning* The subject could form as many or as few piles as he liked, with as few or as many cards as he chose in each pile It is Miller's contention that people will sort cards together according to shared semantic features, thus overlooking the features that would normally distinguish one noun from another Thus by pooling data from a number of subjects the number of times a given pair of nouns appeared together in the same pile (with 50 subjects the theoretical maximum is 50) can be seen as a measure of similarity of the two items The higher the number, the greater the subjects adjudged the similarity of meaning

50 native speakers of Dutch (all either students or staff in the Faculty of Letters at the University of Nijmegen) took part in this investigation The subjects were presented with 17 cards, on each of which was written a sentence illustrating a sense of *breken*. The 17 sentences were those used in the second transferability

experiment. Subjects sorted cards into piles according to their individual notions of similarity. The number of piles made by subjects varied from 2 to 15, with an average of 7 28 and a standard deviation of 2.95. Some subjects finished in five minutes, others in twenty.

From the data produced by card sorting, a matrix of similarity scores was produced, based on the number of times subjects had put any two cards in the same pile. The matrix is as follows·

1	waves														
33	2	light rays													
14	14	3 leg													
14	14	45	4 cup												
3	3	2	1	5	man										
3	3	1	1	41	6	heart									
3	3	0	0	8	15	7	word								
3	3	0	0	8	15	50	8	oath							
4	5	0	0	4	7	20	21	9	ceasefire						
3	3	0	0	4	7	18	19	45	10	strike					
2	2	0	0	7	9	26	25	20	7	11	law				
6	8	6	6	9	11	8	9	7	2	10	12	ice			
4	5	0	0	5	5	5	6	8	2	6	22	13	game		
4	7	3	3	6	9	13	13	15	3	13	8	8	14	record	
7	9	2	2	6	7	4	5	3	0	6	8	6	9	15	voice
18	15	5	7	1	4	5	4	4	3	1	6	6	2	11	16 fall
4	4	0	0	2	6	15	16	34	11	17	6	6	11	4	6 17
															resistance

Table 6: Similarity matrix of meanings of *break*

This matrix was submitted to computer analysis by the MINISSA program (July, 1977 version, developed by E. Roskam, Nijmegen, J. Lingoes, Michigan, and M. Raaijmakers, Nijmegen), which scales the data so as to reveal their underlying structure in terms of an n-dimensional Euclidean representation of *semantic space* (see for instance Caramazza and Grober, 1976, or Henley, 1969). The smaller the number of dimensions the easier it may be to interpret the

dimensions, but the higher the risk of unacceptable stress (Kruskal, 1964), a statistical measure of the 'violence' being done to the data by reduction in the number of dimensions. The stress can be expected to increase as the dimensionality decreases. In our case, two- and three-dimensional solutions can be obtained without any intolerable degree of stress. In the case of the 3-D solution, (see fig. 1) the third dimension to be revealed (and therefore the least important in terms of underlying structure) could not be interpreted and is therefore not shown below. In the 2-D solution (fig. 2), this third dimension, of course, disappears altogether, with only a minimal increase in stress:

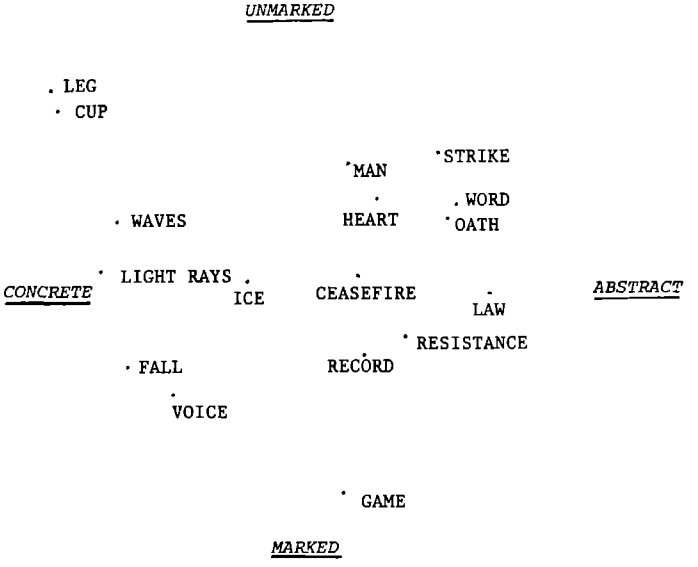

Fig. 1: Multidimensional scaling of 17 brekens - 3-D solutions (after orthogonal rotation of axes)

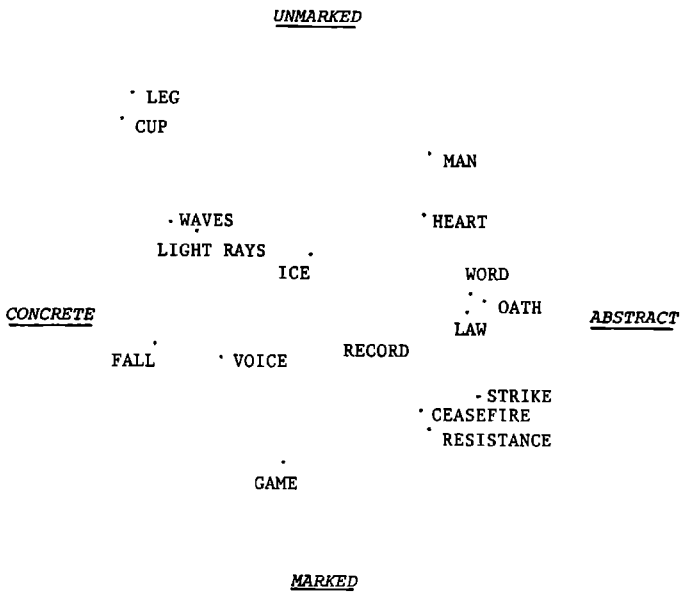

Fig 2. Multidimensional scaling of 17 *brekens* - 2-D solutions (after orthogonal rotation of axes)

After orthogonal rotation of the axes, two possible interpretations of the dimensions presented themselves The first dimension to be revealed, thus the most significant in terms of underlying structure, runs West-East in both solutions This has been labelled *concreteness*, though it could equally well be labelled *high imagery - low imagery* as we move from left to right [7] The North-South dimension has been labelled *markedness* since it appears to arrange the senses according to their relatedness to the primary sense of *breken*. In this interpretation, *to break someone s heart* is simply a metaphorical extension of the primary meaning - the heart, symbol of happiness or whatever, is broken in two It will be clear that this is not the same as saying that senses like *heart* are adjudged very similar to the primary sense On the contrary, the Euclidean distance between *heart* (6) and, say, *cup* (4) is greater than between *cup* and three other items (*waves*, *light rays* and

ice), which are judged more similar to *cup*, yet are more 'marked' than *heart*.

We now have three sets of data:

a) Transferability judgements for nine senses of *breken* made by 291 subjects (NU2,NU1,NUO,UT3,UT1,A6,A5,A4,A3,A2)

b) Transferability judgements for seventeen senses of *breken* (including the original nine) made by 81 subjects (UT3,UT1)

c) Multidimensional scaling solutions for native speaker similarity judgements of seventeen *brekens*(50 subjects).

Clearly, to show that transferability is a function of 'markedness' it will be necessary to demonstrate some kind of relationship between the transferability scores and the semantic spaces revealed by multidimensional scaling First we compare the 3- and 2-dimensional solutions (Tables 7 and 8).

3D meaning	rank order	2D meaning
cup	1	cup
leg	2	leg
light rays	3	fall
waves	4	waves
fall	5	light rays
voice	6	voice
ice	7	game
man	8	ice
game	9	record
record	10	resistance
heart	11	heart
strike	12	man
ceasefire	13	ceasefire
resistance	14	oath
oath	15	word
word	16	law
law	17	strike

Table 7: Rank orders of meanings along a putative *concreteness/ imagery* dimension, three- and two-dimensional solutions, taken from Figs. 1 and 2.

It will be seen that the first six senses in both solutions are clearly *perceivable* senses which is consistent with an *imagery* or *concreteness* structure. The 'markedness' ordering is as follows:

3D meaning	rank order	2D meaning
leg	1	leg
cup	2	cup
strike	3	man
man	4	heart
heart	5	waves
word	6	light rays
waves	7	ice
oath	8	word
light rays	9	oath
ceasefire	10	law
law	11	record
ice	12	fall
resistance	13	voice
record	14	strike
fall	15	ceasefire
voice	16	resistance
game	17	game

Table 8: Rank order of meanings along a putative 'markedness' dimension, three- and two-dimensional solutions, taken from Figs 1 and 2

The two concreteness/imagery orders (Table 7) correlate well with each other (Spearman's rho = 985, significant at < 01), as do the two 'markedness' orders in Table 8 (Spearman's rho = 721, significant at < .01). Since both solutions correlate with each other strongly, and the 'stress' for both is satisfactorily low, both will now be compared to the scores deriving from the transferability experiment

5.5 Correlating transferability data with the data from similarity judgements

a) Predictions for the original nine senses of *breken*.

If we now compare the rank orders of senses along the 'markedness' dimensions with the rank orders derived from the transferability experiments, it will be seen that there is generally a strong correlation between the two:

'Markedness' rank order

	3-D solution		2-D solution	
	S	sig. level	S	sig. level
NU2	21	< .05	17	.05
NU1	21	< .05	17	.05
NUO	23	< .05	21	< .05
A6	27	< .01	23	< .01
A5	27	< .01	27	< .01
A4	29	< .01	25	< .01
A3	30	< .01	28	< .01
A2	27	< .01	21	< .05
UT3	23	< .01	20	< .05
UT1	28	< .01	24	< .01

Table 9: Significance levels of correlations between rank orderings of 'markedness' and rank orderings from group transferability data for nine senses, using Kendall's measure of disarray, S.

b) Predictions for the full seventeen senses.

A comparison of the 'markedness' rank orders and the transferability data for the Utrecht groups yields the following:

	3-D solution	sig. level	2-D solution	sig. level
UT3	.586	< .01	.842	< .01
UT1	.629	< .01	.744	< .01

Table 10: Significance level of rank order correlations between 'markedness' dimensions and transferability data for seventeen senses, using Spearman's rho.

Here it is clear that the two-dimensional solution gives very much better results. However, strike seems unnaturally high on the 'markedness' dimension in the three-dimensional solution. What is more, it is of low transferability. The effect of this item on the correlation is substantial, and if it is removed the resultant rank orders of 16 items correlate extremely highly:

	rho	sig. level
UT3	.820	< .01
UT1	.867	< .01

Table 11: Significance level of rank-order correlations between markedness dimension from 3-D solution and transferability data after the removal of strike, using Spearman's rho.

If we now calculate the rank-order correlations between the *concreteness/imagery* dimensions and the transferability data for the seventeen items (totalled up from UT3 + UT1 for convenience), there is virtually no correlation at all (3-D solution and transferability, rho = 0.057, n s , 2-D solution and transferability, rho = 0 129, n s).

5.6 The effect of the target language on transferability

The preceding discussion will have indicated that transferability is theoretically independent of the TL, since it is a direct reflection of the 'markedness' of a NL item If the TL's role is to partially determine the cut-off point in a transferability scale below which transfer will tend not to occur, but not to affect the ordering of the scale, then the scaling solutions used here should also correlate with the translation preferences of Dutch learners of German.

40 Dutch learners of German at Nijmegen (1st and 3rd year-students) were given the nine-item transferability test The instructions were modified to take German grammar into account so that subjects could accept either *zerbrechen* or *brechen* as a translation of *breken*, or reject them both.

The resulting transferability rank order, based on acceptances, is.

	% acceptances
1. cup	100
2. leg	100
3. heart	92.5
4 word	62.5
5. man	50
6. waves	37.5
7. voice	37.5
8. record	35
9. fall	22 5

Table 12 Transferability rank order for Dutch learners of German

Correlation with the 'markedness' rank orders is as follows (Kendall's S):

	3-D	sig.	2-D	sig.
S	24	< .01	24	< 01

Table 13: Significance level of correlations between rank orders of 'markedness' and rank orders from transferability data for nine senses, using Kendall's measure of disarray, S

These results support the hypothesis that transferability ordering is independent of a given TL.

5.7 Discussion and conclusions

The results given above show that for *breken*, the transferability of its senses can be predicted from analysis of similarity judgements, which can be used to construct a low-stress semantic space of two or three dimensions. Two of the three dimensions can be interpreted as, on the one hand, high-low imagery or concrete-abstract, and on the other, 'markedness'. Whereas it is perhaps fairly clear what the former dimension entails, a little space should now be devoted to discussion of the latter.

A number of speculations can be made to account for the above findings. The first of these concerns the primary sense of *breken/break* as represented in such a sentence as *He broke the cup*. The closer a sense is to this primary sense, the more transferable it should be. The problem lies with the definition of 'closeness' one is going to adopt, since similarity itself does not directly predict transferability. Since senses of *broken* used in non-concrete environments (*hearts, man* etc.) can be more transferable than concrete *brekens* like *voice* or *fall*, it is clear that the structure underlying the data from card sorting reveals a more complex arrangement of items It is insufficient to say that the 'markedness' dimension is the other (weaker) component underlying the sorting data, without some explanation being offered as to what it is and why it should correlate so well with transferability data. One possibility would be that primary *break*

can be analysed into a bundle of semantic features like *of solid, brittle, needs force*. Whenever *break* is collocated with an object which itself can match these features, then the primary meaning will be inferred. Metaphorical interpretations of senses will be postponed and are subsidiary to feature matching. In this scheme, certain metaphorical senses would violate the feature matching condition to a lesser extent than some concrete senses *Waves*, for instance, are neither solid nor brittle, though they can be *metaphorically solidified (A tidal wave like a solid wall of water)*; *fall* would seem to share no obvious features with *c.p*, apart from *perceivable*. *Word* would require first that one *solidify* it - a difficult conceptual task not to be found with *heart* or *man*. The question with this kind of approach is how far one can go positing *metaphorical solids*. The greatest problems will occur with senses like *He broke the news* or *The storm broke*- unless it would be simpler to claim homonymy for the difficult cases - a tempting claim.

Caramazza and Grober (1976) have argued for an underlying core meaning for all the senses of *line* they studied. They claim that surface meanings would be built up from a *core* meaning by a number of instruction rules The greater the number of rules the more complex the processing involved with that sense, and the less likely it is to be cited as 'typical' or 'representative' Miller's (1978a) approach is similar. Using *line* as his example, he writes· "the problem is to characterise the relations between this core sense and all the particular senses of *line* listed in the dictionary Those relations should not be specified to *line*, but should apply to other semantic extensions elsewhere in the lexicon, in which case they might be formulated as rules that people learn when they master their English vocabulary" (p. 101)

Miller's discussion, which closely resembles Kelly and Stone's (1975) in many respects, revolves round the question of how one goes about selecting the appropriate rule or rules for correct interpretation Having a limited number of core concepts which can be summoned up by a particular context would, he claims, be far more plausible than having to store endless separate meanings for each occurrence of the word in a different context. The context,

linguistic and pragmatic, will effectively select the right sense for us - as Miller says, "it would be distinctly odd to have to execute a disambiguation routine to discover whether the context is the nautical kind in which *line* might be understood as *rope*" (p. 102) when one is discussing the rescue of a drowning woman. Miller goes on to say that the apparent polysemy of *line* may be due to lexicographers including "a lot of contextual information that is really not part of its meaning" (1978a, p. 102). He notes that

> "the inferential process seems more plausible than an ad hoc list of .. objects ... In some cases the set of admissible subjects or objects of a verb seem to form a coherent class that can simply be remembered. But in other cases - and probably in most cases for young children - inferences based on practical knowledge and prevailing circumstances are the ultimate court of appeal". (1978b:409-10. See also Kelly and Stone, 1975.65ff)

One could invoke here the concept of [illegible], which is discussed in e.g. Kelly and Stone (1975), and Miller (1978b), where the precise meaning of a word like *good* is determined by the noun it qualifies, e.g *A good knife cuts well, A good chair is comfortable*, etc. Miller (1978b) writes:

> "'Good' can select a salient feature of the meaning of its noun and assign a positive value to that feature".
>
> (p. 405)

Presumably one might try to argue on similar lines for *break*, though it would be difficult to account for every sense of the word in this way (consider for instance a principle like 'break puts an end to the continued existence of some given entity' - how would this apply to *She broke the news* or *The storm broke* unless we invoke catastrophe theory!).

Perhaps there is a compromise position which tends to favour Miller's 'static' position rather than Caramazza and Grober's 'dynamic' one. This would be as follows: senses are learned as part of conventional collocations[8] in some cases, and as generalisable concepts in others. Thus the child can learn to generalise from *breaking cups* to all manner of 'breakable' objects. But other senses are only evoked in collocations where the meaning is ritualised, as in *To break one's word*, or *To break somebody's*

heart. It will not normally be necessary for there to be a cognitive link between these breaks and generalisable ones However, when the child, or learner of a foreign language, or reader of some idiosyncratic dialect, comes across what is for him a novel environment for a word, and an attempt is made to interpret it, then rapid scanning of core concepts will take place, with the help of pragmatic knowledge - the 'ultimate court of appeal' I would argue against the generalisability of all core concepts to new cases, on the basis of differences between typologically close languages Take the sense of break as typified in To break one's word, the law, an appointment, a contract, a code of behaviour, a ceasefire, etc. etc , i e to violate some sort of agreement, or set of rules binding parties. In Dutch, the generalisability of breken is limited to agreement, contract and word. The generalisability of a sense to new cases will be ad hoc- first we learn its meaning, then we have it available for building up interpretation rules But unlike objects like cup the generalization may only be receptive, that is, we do not use it to produce new forms before we have heard them and they have impinged on our consciousness [9]

To illustrate the point, let us look at what would be a novel use of break in English, but one which is interpretable nonetheless·

His life broke

By itself, a number of interpretations might be possible for this use of break. Put in the following context, however, the interpretations seem, on the basis of informal elicitation, to reduce to one or two

Finally, at the age of 21, his life broke

This sentence has been interpreted for me either as meaning changed for the better or changed for the worse. Yet if we simply change '21' to '96'·

Finally, at the age of 96, his life broke

the interpretation shifts from changed to ended- in other words He died. Only pragmatic considerations can lead to this shift in interpretation (which also highlights the potential ambiguity of

life - period of biological activity or history of personal events in that life) The main point is still that some sort of drumming up of available concepts must take place (combined with practical knowledge) to reach an appropriate interpretation However, the generalisability of such concepts to potential collocations is restricted by convention For this reason, lexicographers should not be put off by psycho-economists from preparing their involved dictionary entries for words like *line* or *break*. (It would be interesting to see what kinds of overgeneralization occur in children's 'neo-collocations')

A further point to consider is that there may be no single underlying organizational system for handling the diverse core meanings of a polysemous word For *break* one could argue for a primary sense to which all the others are variously related + a number of core senses This primary sense is a surface, not an underlying one For *line*, an underlying core concept is perhaps more likely, since it is difficult to agree upon a primary sense [10] And for *eye* (Kellerman, 1980, Chapter Six), there could only be a primary sense, features of which are variously shared with extensions of meaning to concrete objects (*eye of needle, electronic eye*, etc) Perhaps in the case of *break* we should even be thinking in terms of actual homonymy, with *cups, hearts*, and *legs* representing one discrete meaning There is certainly a degree of discomfort about the entry for the verb *break* in Kelly and Stone's (1975) *disambiguation dictionary*, viz

Sense 1 To fracture, split, stop or cause to stop functioning, cause a division or change, enter forcibly, escape, begin suddenly, interrupt, etc

Sense 2 (Idiom) break the news

Sense 3 (Idiom) break a law

Since Kelly and Stone argue for a small number of core senses being appropriately interpreted in situ, it is a pity that their own entries for this verb are woefully inadequate If there is a unifying sense in Sense 1, it escapes the present author A more interesting proposal of theirs is 'to develop evidence of parallel

sense-development in separate languages ( ) as this would suggest the operation of 'cognitive universals' (p 77)

This last proposal really brings us back to the experiments described above Historical comparisons of meaning extensions are complex, and ways must be found, as Kelly and Stone acknowledge, of eliminating the effects of interlanguage borrowing However, we can make use of the uninformed translations by learners of languages from NL to TL to establish the transferability of senses These translations, though often at variance with the facts of diachronic development, nevertheless may be revealing of the structure and limits of polysemy, and many also help to settle the question of whether a small number of core concepts and interpretation rules versus a fully (or over-) specified description of environments of occurrence in the mental lexicon is the right one In this respect, the results above show that the generalised intuitions of learners do not allow the same degree of sense generalization for English as for Dutch, despite the fact that all but a few senses in the experiment could be successfully translated between both languages, and also despite the fact that English has extra senses not shared by Dutch That this is so must be due to precisely the kind of cognitive mechanism that potentially generalises senses to new environments receptively or productively Since cross-language comparisons only incidentally provide insight into the language faculties of individuals, it is surely essential to tap the intuitions of native speakers who also happen to be learners of foreign languages as well

NOTES TO CHAPTER FIVE

1 This is a very slightly revised version of an article in *Studia Anglica Posnaniensia* 14, 1982, pp 197-220 A preliminary version entitled 'Giving learners a break, native language intuitions as a source of predictions about transferability', appeared in *Working Papers on Bilingualism* 15, 1978, pp 59-92

2 Note that contrary to normal practice, 'markedness' as used in this paper does not presuppose a purely binary opposition 'marked/unmarked' It is to be understood as a psycholinguistic concept, applicable to some defined linguistic system (i e syntactic structures or, as here, the senses of polysemous words) which is itself gradable in terms of 'most marked - least marked' Alternative terms have been used by the author in earlier papers, but have now been rejected as unsatisfactory for one reason or another The term 'markedness may not be the last word either

3 It will also follow that the more 'marked' an item, the more likely it is to be adjudged specific It should again be noted that specificity is an all-or-nothing statement about behaviour (*transferred or not transferred*) while *transferability* is a statement of probability

4 This is a convenient overgeneralization Idiomatic expressions vary in their semantic transparency and there appears to be some moderate correlation between their transparency and their acceptability in translation

5 Concreteness *per se* does not determine the primary meaning of a word 'Delve', for instance, has a literal meaning (*When Adam delved and Eve span, Who was then the gentleman?*) which must be seen as secondary to its contemporary figurative meaning of research (into), investigate.

6 A similar test of the 17 breakens requiring full translation of the Dutch sentences was conducted with 17 first-year students (1979) By scoring each time break was used as a translation for each sense, an acceptance score rank order could be established, as was the case with the Utrecht test The correlation between the two

orders was .837, $p < .01$, and with the 2-D solution, .736, $p < .01$, and the 3-D solution .723, $p < .01$.

7. Paivio, Yuille and Madigan (1968) have shown that judgements of concreteness and imagery in nouns show high correlations.

8. As Kelly and Stone (1975) point out, we do perhaps suffer by an obsession with analysis of meaning at the morpheme level. They say "dictionaries often exaggerate the polysemy of a word by attributing to it the meaning of phrases in which it appears - both idiom and common locutions".

9 Bolinger (1976 8), in a highly entertaining and instructive article, makes the general point very convincingly. "The question is, why do we not generate *an extended time ago if we generate a lifetime ago, and why do we not generate sometime else if we generate somewhere else? It is not because the generative mechanism is lacking. *I suggest that at least in part we do not do it because we have not heard it done. We have no memory of it.* Also. "...learning goes on constantly - but especially with young children - in segments of collocation size as much as it does in segments of word size, and that much if not most of our later manipulative grasp of words is by way of analysis of collocations" See also the ensuing discussion of tu bear on p 9 of this same article.

10. A quick survey of 9 dictionaries reveals that 7 give as their first definition of line 'piece of string, thread, cord', etc. Only two list 'mark or stroke' first

6. AN EYE FOR AN EYE[1]

Crosslinguistic constraints on the development of the L2 lexicon

6.0 Introduction

This chapter concentrates on methodological problems in the study of crosslinguistic influence in second language acquisition. These problems concern the selection of an appropriate body of data and the purpose to which that body of data is put It is argued that formal experimental techniques eliciting learners' intuitions are indispensable if certain research questions are even to be formulated, let alone answered. Although such techniques require proper controls, and the data they provide require responsible interpretation, in the case of the learner's L2 lexicon it is often only by the presentation of quite specific tasks that it becomes possible to explore with the necessary delicacy the way in which knowledge of the structure of the L1 interacts with the learner's developing perception of the L2.

In a later section two experiments will be described which explore the relationship between native speakers' judgements of metaphoric uses of the Dutch word oog, 'eye' and the English word head, and learners' judgements as to the probability of the existence of the equivalent metaphors in the L2 It is not claimed that the particular model posited below will accurately predict second language performance (except in a probabilistic sense), no model can do that yet Rather it is an attempt to characterize the development of knowledge underlying potential performance, and more specifically, to characterize the constraints on the development of that knowledge imposed by the L1.

6.1 On the need for the formal elicitation of data

One concern of second language acquisition research, indeed of linguistics in general, is the manner in which data are to be collected. Ideally, we might want all our data to come from 'natural' conditions, since it has been argued that:

> our goal is . . to observe the way that people use language when they are not being observed . . (Labov, 1975:34n)

the Observer's Paradox. Failing this ideal, we may have to settle for (quasi-)spontaneous data deriving from observed interaction between learners, and between learners and native speakers. In other cases it may suit us better to use on-line experimentation or to tap metalinguistic intuitions via judgements of one kind or another. While all these possible data sources serve to enrich considerably the potential array of corpora available for analysis, they also commit us to a proper recognition of the constraints on hypothesis building that each type of corpus imposes on us.

The assembly of a relevant corpus obviously depends on what the object of study is. In some cases a corpus is easy to put together, especially if we are after linguistic phenomena which are highly frequent and accessible in a wide variety of contexts (e.g. phonological, morphological and simple syntactic or thematic structures). However, some interesting phenomena are less accessible and require special elicitation if they are to appear in researchable quantities. If this observation seems self-evident, it is only made to point out that spontaneously derived data do not necessarily provide the researcher with the wherewithal to examine the object. The elicitation of non-spontaneous data does at least allow us to deal with quite specific areas of language under quite specific conditions dissected away from all irrelevances. Such 'clean' data come from rigorous testing procedures; techniques for gathering such data have been used for years.[2]

Furthermore, since the present focus of interest is on constraints on crosslinguistic influence, it becomes difficult to see how, in the matter of lexis, we can use corpora of spontaneous speech to cope with phenomena which are likely to be conspicuous by their *absence* in such corpora. Their failure to appear may only emphasize the inadequacy of the elicitation procedure itself. This possibility requires us to look beyond performance data to more theoretical notions of potential for transfer, or *transferability*, and to do this requires a different approach. It may therefore be difficult to extrapolate directly from the findings of formal elicitation procedures (e.g. acceptability judgements) to predictions of language in use. Instead, claims can only be made about the learner's knowledge underlying performance. The

distinction is sometimes elusive, but as Sharwood Smith (1986) argues, it must be insisted upon. Consequently, since we are in no position to predict actual behaviour (that is, what will happen with a specific structure on a specific occasion) we should limit ourselves to predicting tendencies in language in use (*pace* Faerch, 1984).

In other words, it is only useful to say that transferred structure *i* appeared in context A, but not in context B, if we can already say that if *i* were to appear at all, it would appear more often than structure *j* (i.e. *i* is more transferable than *j*). In this way we can attempt to cut a swathe through the large number of performance variables that intervene between knowledge and the processing of that knowlege.

The transferability of a structure, then, is the probability with which it will be transferred to an L2 compared to some other structure or structures. Such statements of probability only make sense if the structures they refer to have some kind of linguistic cohesion. We would not want to compare the transferability of a specific complementation device with that of an idiomatic expression if we wanted to say anything interesting about crosslinguistic influence. We might well compare the transferabilities of various idioms in themselves (e.g. Kellerman, 1977), or the transferabilities of different types of complementation (cf. Jordens, 1977).

Elsewhere (e.g. Kellerman, 1978, Chapter Five) I have claimed that transferability can be established solely on the basis of L1-specific characteristics which are independent of the L2. The role of the L2 in the transferability of a structure will be determined by the learner's perception of its relatedness to the L1 in the given structure domain (his 'psychotypology', Kellerman, 1983). An L2 perceived as 'close' will boost the likelihood of transfer, an L2 perceived as 'distant' will depress it, but the transferability of structures remains constant. Thus if *i* is more transferable than *j*, and the L2 is perceived as close, then both *i* and *j* may be transferred. If the L2 is perceived as distant, then only *i* may be transferred, or not even *i*. In theory, however, we should not find *j* being transferred more than *i* in an indefinitely

large and unbiased corpus (i.e. where any structure has any equal opportunity of occurrence). Of course, the establishment of measures of transferability for L1 structures brings with it the strong requirement to explain why it is that some structures in a given domain are more transferable than others. This is where the whole question of crosslinguistic influence becomes interesting, particularly with regard to the lexicon, where attempts to predict and explain learner's intuitions are thin on the ground (cf. Meara, 1984).

The 'eye' and 'head' experiments address transferability directly by seeking to measure the likelihood that *i* will be more transferable than *j* without claiming that *i* will be transferred and *j* will not, or that both will, or that neither will. The sources of predictions are intuitions about the L1 and take the form *Is i greater than j according to some specified dimension?* These predictions are then compared with transferability judgements of the type *Is i more likely to be transferable than j?* without committing subjects to a categoric statement of whether *i* and *j* are indeed transferable to a given L2. It is the claim of this chapter that transferability can indeed be established entirely on the basis of the learner's knowledge of his native language, and that the establishment of these probabilities will have validity for any given L2.

6.2 Previous research

In Kellerman (1982), the polysemous Dutch word *breken* (to break) was studied with a view to investigating:

(a) the native speaker's semantic space (see, e.g. Clark and Clark, 1977) containing different but related senses of the word; and

(b) the relationship between this space and the probability assigned by learners to each of these senses that they could be represented in English (or German) by the *primary counterpart* (Arabski, 1979) of *breken*, namely to *break* (or *zerbrechen*).[3] This probability is termed the *transferability* of a sense.

It was shown that the structure of such a semantic space could be represented in two dimensions, the first of which correlated well

with learners' judgements of the type 'sense$_1$ > sense$_2$ > sense$_3$ > ... sense$_n$' (where > means is more likely to be represented by the primary counterpart than ...) The arrangement of senses along this dimension (labelled 'markedness' in the terminology of Kellerman, 1982) was hypothesized to correspond to how much a sense shares semantic attributes with the prototypical sense of *breken* Coleman and Kay (1981) define 'semantic prototype' as follows

> a semantic prototype associates a word or phrase with a prelinguistic cognitive schema or image, speakers are equipped with an ability to judge the degree with which an object (or the internal representation thereof) matches this prototype schema or image

For *breken/break*, the prototypical sense would be something like 'to (cause to) separate into parts suddenly or violently, but not by cutting or tearing' (*Longman Dictionary of Contemporary English*, Procter, 1978) This degree of attribute sharing is apparently minimally affected by considerations of *concreteness* or *imagery* which, it was postulated, is what the second dimension consisted of This means that senses of *breken/break* as represented by *She broke his heart* or *The accident left him a broken man* are seen as more related to the 'prototypical sense' than such *concrete* or *high imagery* senses as contained in *The waves broke on the shore* or *The tree broke his fall*,[4] presumably because in the first two cases it is the prototypical sense that is being metaphorized The arrangement of senses along the 'concreteness' or 'imagery' dimension showed virtually no correlation with the transferability judgements

An analysis of raw similarity data[5] shows that they do not make good predictors of transferability By rank-ordering the senses according to their similarity to the prototypical sense (as in *He broke his leg*) and comparing this rank order with the transferability ordering, we obtain a correlation coefficient, Spearman's *rho*, of 0 41, $p < 05$, which is frankly unexciting, though significant Using derived similarities, i e the Euclidean distances between the prototypical sense and the remaining senses in the two-dimensional space, produces a similar correlation of 0 40 [6] Transferability and prototypicality orderings correlate with a *rho* of 0 837, $p < 001$ Clearly, then, whatever underlies

prototypicality, it is not similarity *tout court*.

Hoeks (1984), using similarity ratings for common Dutch nouns each with at least two distinct meanings (e.g. *loods* = 'river pilot', 'warehouse', *roos* = e.g. 'rose', 'bull's eye') found that the greater the similarity between the meanings as perceived by Dutch native speakers, the more likely a single English translation equivalent would be selected by Dutch learners of English. When the meanings of the L1 noun were seen as very different, more than one noun tended to be selected in English to cover the meanings. The L2 error patterns reveal that while L2 knowledge clearly does play a role, there is still evidence of the transfer of semantic distance to English. Which of the two meanings of the Dutch noun would be allocated to which English lexical item was very largely predictable from a free production test in Dutch in which subjects were asked to provide sentences illustrating the first meaning they thought of for each noun (which may be an indication of a greater perceived frequency). Thus the majority of subjects (75 per cent) provided a sentence containing a 'warehouse' interpretation of *loods*, which meaning was then considered to be the prototypical one (and at the same time showing that animacy is not a factor in determining prototypicality).

6.3 The 'eye' experiment

In Kellerman (1979a,b) it was suggested that prototypicality might be a complex of factors of which subjective frequency (or perhaps familiarity) of the sense could be one. The reasoning behind this is as follows:

If there are two senses of a polysemous word, *i* and *j*, such that both are judged equally similar to the prototypical sense according to some attribute (e.g. shape or function), then they will be equidistant from the prototypical sense in an *n*-dimensional semantic space. However, it has already been shown that similarity *per se* does not predict transferability, i.e. does not correspond to the ordering of senses along the prototypicality dimension. The frequency of an item should therefore interact with similarity, so that a greater frequency should contribute to the assignment of a higher degree of prototypicality of a sense. The *eyes on a*

peacock's tail may be used to illustrate the point. Whilst it is easy to appreciate the similarity of these markings (cf. the protective wing markings of certain moths) to the *organ of sight*, and that they are somehow closer to the latter than the *eye of a needle*, it is also clear (at least to the people I have consulted) that the latter *eye* is also more frequent in the world of discourse. As we shall see, when it comes to transferability, *eyes of needles* have it over *eyes on a peacock's tail*. What would be claimed here is that this is due to a perceived frequency effect Thus a sense which is in everyday language used to denote some common object should, *ceteris paribus*, be viewed as more transferable than one which refers to some more esoteric object. This is putting the position at its most extreme for the sake of clarity.

The 'eye' experiment was designed to test whether judgements of similarity and frequency can be used to predict the transferability of polysemy. Like much of the work on subjective frequency (see e.g. Howes, 1954; Carroll, 1971; Ringeling, 1984), prototypicality (Rosch, 1973), and semantic space (e.g. Henley, 1969), the material used in the experiment consists of concrete nouns.[7] This choice brings with it immediate methodological benefits in terms of similarity and frequency judgements being far easier to make with concrete nouns than with mixed concrete and abstract senses of a verb like *break*.

As in the earlier paper (Kellerman, 1982), the focus of interest here is polysemy, in this case in the form of the concrete *anthropomorphic* or body-part metaphor (Ullmann, 1977:162). As Ullmann says:

> A word can be given one or more figurative senses without losing its original meaning .. In this way a number of metaphors may ... 'radiate'[8] from the central sense. The word *eye*, for example, may[9] be applied to a wide range of objects reminiscent of the organ.

Given that *eye* in English is polysemous, as is its equivalent in Dutch, *oog*, it is a particularly suitable word for investigating crosslinguistic influence.

Hypothesis

The transferability of a sense i (relative to senses j,k,l etc.) of a L1 polysemous word to a L2 polysemous word will be a function of its perceived similarity to the prototypical sense of the L1 word and its subjective frequency in the L1 (again relative to the values of these variables for senses j,k,l, etc.). This hypothesis may be expressed by a simple formula:

$$T_i : T_j = (Sim \times Frequ)_i : (Sim \times Frequ)_j$$

where T = transferability, Sim = similarity, and Frequ = frequency. Note that this formula is presented as if there were absolute values for T, Sim, and Frequ. This is not the case, of course, as we have argued above: the transferability of i can only be understood in terms of some other sense j, via a comparison of relative values for Sim and Frequ. The formulation of this hypothesis entails a method of investigation in which senses are compared to each other in terms of the three variables, as is outlined in the next section.

a) Material

The polysemous Dutch word oog (*eye*), like other Dutch and English words referring to parts of the body, also exists in a number of concrete extensions of meaning. Oog probably has more extensions of meaning than other such Dutch words in common use, excluding their appearance in idiomatic expressions (*To keep an eye on someone*), diminutives (*eyelet*), or fixed collocations (e.g. *hooks and eyes*). Six common senses are represented by the following phrases:

Het menselijk oog	(The human eye)
Het oog van een aardappel	(The eye of a potato)
Een electronisch oog	(An electronic eye)
De ogen op een pauwestaart	(The eyes on a peacock s tail)
Het oog van een naald	(The eye of a needle)
De ogen op een dobbelsteen	(The spots, pips on a dice)

(N.B.: the italicized English words will serve as mnemonics for the Dutch senses.)

It was felt that apart from oog in the sense of organ of sight, none of the five senses which could be translated by eye would be explicitly taught. This should mean that intuitions about transferability would be at work rather than actual L2 knowledge, though L2 knowledge effects cannot be ruled out *a priori*.

b) *Subjects*

35 Dutch first-year students of English at the University of Groningen participated in the experiment, which was conducted in normal class time.

c) *Method*

The six senses of oog given above, incorporated into defining phrases, were presented in three separate, successive, tests The senses were arranged in all possible pair combinations [(N(N-1)/2)=15]. Each pair was written on a separate sheet of transparent acetate film, one phrase above the other and projected via an overhead projector onto a screen.

The three tests, all forced-choice preference tests, were as follows:

A. The Translation Test
B. The Similarity Test
C. The Frequency Test

The subjects were divided into four groups. For each group the presentation of tests, the presentation of pairs within the test, and for two groups the order of items in each pair, were changed to minimize unwanted biases due to presentation conditions. Subjects wrote their responses anonymously on blank sheets of paper Responses were indicated by either 1 or 2, signifying whether subjects preferred the top or bottom phrase in each pair Papers were collected at the end of each test. The instructions for each test were given orally and were as follows.

A. The Translation Test

Subjects were asked to select which sense in each pair was more likely to be rendered by oog in English. Note that subjects were

not asked if the senses actually could be translated by *eye* since a pilot study had shown such judgements to be unreliable and difficult to make

B The Similarity Test

Here subjects were asked to choose which sense of a given pair was seen as *more similar* or *closer in meaning* to the meaning of *human eye* Needless to say, all pairs containing *human eye* were removed in this test, hence the test consisted of 10 pairs

C The Frequency Test

Here subjects had to choose which sense was in their opinion the more 'frequent' in ordinary everyday language

All instructions were presented orally with examples given first using other body-part words (e g *nose*) Presentation time for each pair in the experiment was approximately 5 seconds (which was more than enough).

Results

Tables 1-3 show the preference scores for each pair of senses for each test At the intersection of any row or column the table should be interpreted as the item in that row is preferred to the item in that column by **n** subjects (column and row headings are mnemonics for the Dutch senses) In Table 1, for instance, it will be seen that six people found *oog van een aardappel* more likely to be translated by *eye* than *oog op een pauwestaart* would be Twenty-nine people thought otherwise (6 + 29 = 35 subjects) Only in two isolated cases is *menselijk oog (human eye)* considered less translatable by *eye* than other senses - we may assume these are aberrations

Table 2 gives the Similarity frequencies derived from the Similarity test, while Table 3 gives the Frequency scores

	potato	peacock	electronic	human	dice	needle
potato	-	6	0	0	10	3
peacock	29	-	11	0	22	14
electronic	35	24	-	1	32	24
human	35	35	34	-	35	34
dice	25	13	3	0	-	7
needle	32	21	11	1	28	-

Table 1: Preference scores for Transferability test

	potato	peacock	electronic	(human)	dice	needle
potato	-	3	6	(-)	20	17
peacock	32	-	13	(-)	31	30
electronic	29	22	-	(-)	28	29
(human)	(-)	(-)	(-)	-	(-)	(-)
dice	15	4	7	(-)	-	18
needle	18	5	6	(-)	17	-

Table 2: Preference Scores for Similarity test

	potato	peacock	electronic	human	dice	needle
potato	-	20	11	0	12	7
peacock	15	-	13	1	10	7
electronic	24	22	-	2	15	8
human	35	34	33	-	34	33
dice	23	25	20	1	-	11
needle	28	28	27	2	24	-

Table 3: Preference scores for Frequency test

These preference scores may be used to calculate ratio scale values for each item.[10] From these ratio scale values, theoretical preference scores for Transferability will be calculated on the basis of the interaction between the Similarity and Frequency scores. The details of the calculations (which make use of Luce's Choice Theory, Restle, 1971) need not concern us here.[11] If the hypothesis is correct then the theoretical Transferability preference scores, derived entirely from L1 judgements, should not differ significantly from the observed Transferability preference scores of Table 1. Table 4 presents the observed and theoretical Transferability scores.

	Observed (O)	Theoretical (E)	X^2 =
potato > peacock	6	5.765	0.010
peacock > potato	29	29.234	0.002
potato > electronic	0	3.265	3.265
electronic > potato	35	31.735	0.336
potato > dice	10	11.807	0.277
dice > potato	25	23.193	0.141
potato > needle	3	6 529	1.907
needle > potato	32	28.471	0.437
peacock > electronic	11	12.000	0.083
electronic > peacock	24	23.000	0.043
peacock > dice	22	25.226	0.413
dice > peacock	13	9.773	1.066
peacock > needle	14	18.817	1.233
needle > peacock	21	16.183	1.434
electronic > dice	32	29 115	0.286
dice > electronic	3	5.885	1.414
electronic > needle	24	24.160	0.001
needle > electronic	11	10.840	0.002
dice > needle	7	10 870	1.378
needle > dice	28	24.130	0.621

Table 4: Comparison of observed and theoretical Transferability scores X^2 = 14.349 (df = 10); probability under H^o that $X^2 > X^2$ square = 0.20 - 0.10

Thus there is no reason to reject the null hypothesis that the observed and theoretical Transferability scores will not be significantly different. Further calculation shows that neither Similarity nor Frequency are adequate predictors of Transferability in themselves, nor when added together. In all three cases a comparison of scores yields X^2 significant at < 0.001.

A replication experiment, this time using the polysemous word head (e.g. head of a poppy, head of a nail) was conducted with 89 English secondary school learners of French and German (ages

16-17). The learners of French were at two different levels of proficiency, one group (N = 48) having had one more year's tuition than the other (N = 23). The German group numbered 18. Instructions were essentially the same as in the previous experiment, with subjects giving native speaker judgements and judgements about the foreign language equivalents of *head*, in this case Tête and Kopf. A list of the senses of *head* used is given in Appendix A. For the sake of brevity the preference scores will not be given; the statistical treatment of the theoretical and observed scores is presented in Appendix B

The relation between theoretical and observed Transferability scores is not as striking in the *head* experiment as in the *eye* experiment. In fact, unlike in the previous experiment, the null hypothesis is clearly *not* supported. Three comments on this state of affairs are appropriate. Firstly, the high X^2 values are caused by a relatively small number of pairs only. Most of these pairs contain either 'head on a glass of beer' or 'head on a boil' as one of the items Secondly, the X^2 test is an extremely rigorous statistic to apply to these kinds of data, with the null hypothesis being susceptible to rejection with even quite small differences between theoretical and observed scores.

The third point to be made is that, given the rigour of the statistical treatment, it would be premature to dismiss the model. To do so would be to run the risk of committing a Type Two error (i.e. rejecting the *correct* hypothesis for the wrong reasons, as distinct from a Type One error, where one accepts the *incorrect* hypothesis for the wrong reasons). This point can be neatly demonstrated by performing correlational statistics on the data. In the case of the *head* data, observed and theoretical scores correlate highly in all three groups ($\underline{r}$ = 0.7612, French group, higher proficiency; $\underline{r}$ = 0.8247, French group, lower proficiency; $\underline{r}$ = 0.7908, German group: all significant at < 0 0001). The use of correlational evidence would thus overwhelmingly support the null hypothesis, at the same time conveniently masking its weaknesses.

6 4 Discussion

The above results indicate that native judgements of similarity and frequency can be used to approximate to judgements of transferability for body part metaphors Furthermore, and consistent with previous research, there does not appear to be any obvious effect either of nominal proficiency or of the particular pairing of target and source languages It is of course difficult to determine exactly what is being measured when one asks for judgements of similarity or frequency (see e g Ringeling, 1984) In the former case it is inevitable that such judgements are not made according to a single attribute dimension In the first experiment, *electronic* is judged more similar to *human* than all the other senses, presumably on the basis of its shared function with the organ of sight, an attribute that none of the other senses has *Peacock* is seen as more similar to *human* than *dice*, *needle* or *potato*, presumably on account of colour and marking, since shape is a shared feature Even though the similarity scores for *electronic* and *peacock* with respect to other senses are roughly equal, when the two senses come to be judged for similarity, it is *electronic* that is seen by the majority of subjects as closer to the primary sense Presumably function is the key element in this choice too, while the clear preference for *peacock* over *dice*, *needle* and *potato* could well reflect the relative ease of decision when a principal attribute, shape, is shared between the senses to be judged Since function can only be attributed to *electronic* (which also has the attribute of shape)[12], judgements of similarity may be complicated by a clash of attributes

A further point concerns the problem with frequency judgements alluded to above One cannot tell whether such judgements refer to the experienced frequency of linguistic occurrence, or of the objects themselves, or, more seriously, the frequency (linguistic or otherwise) of the object with which each sense of *eye* or *head* is associated In other words, it is conceivable that subjects are actually judging the frequency of potatoes, peacocks, dice, beer, golf clubs, etc in their

experience (cf. Tversky and Kahneman, 1973).[13] For instance, in the case of the second experiment, most of the most striking mismatches to the model involve judgements about the 'head on a glass of beer' or the 'head on a boil'. Since all the subjects in this experiment were males in their late teens, it may well be that this unexpected effect is in some way related to the psychology and physiology of the adolescent. This is a question that is in need of 'further research'. Nevertheless, whatever the judgements allude to, it is clear that they are generally stable and consistent [14]

While it would be very difficult to find confirmation for the results reported in this paper in a corpus of natural speech data, it might be possible to look for correlates of these findings in another way, i.e via on-line tasks in the L2. Such a procedure would be in accord with Caramazza and Grober's (1976) study of the word *line*, in which both reflective and on-line tasks were used.

Finally, it is not claimed that transferability can be equated in some simplistic way with order of acquisition, with less transferable senses harder to acquire in the L2. The only suggestion to be made in this respect is that some senses may be acquired for free, so to speak, by virtue of their existence in the L1 and the operation of universal generalization processes within the learner. Others, the more idiomatic senses, may require positive evidence in the L2 before they can be acquired, if they are already instanced in the L1, one token may be enough. In the spirit of experiments by Gass (1981) and Eckman (1984) on acquiring relative clauses in a second language in accordance with the Noun Phrase Accessibility Hierarchy (Keenan and Comrie, 1977), it would be interesting to see whether specifically teaching learners the least prototypical senses of a word would enable them to acquire without positive evidence the more prototypical senses; the teaching of more prototypical senses should not have a beneficial effect on the acquisition of the less prototypical ones.

6 5 Conclusion

As far as the data from these experiments are concerned, it seems that a simple multiplicative interaction between judgements of similarity and frequency could predict transferability judgements in the overwhelming number of cases It seems almost trivial to suggest that further experimentation is needed with more extensive material, however, the principal claim of this paper is that experimentation of this kind, where there are rigourous controls on the precise task subjects have to carry out, avoids some of the traps some researchers have unwittingly fallen into because they have assumed that subjects must have been judging what they wanted them to judge Furthermore, this form of experimentation allows one to examine underlying L2 knowledge in a way that would not be possible through the scrutiny of a corpus of spontaneous speech, permitting evaluation of the hypothesis that certain aspects of crosslinguistic influence can be successfully predicted and explained

NOTES TO CHAPTER SIX

1. This is a much revised and extended version of '*Oeil pour oeil*', which appeared in French translation in *Encrages*, 1980, pp.54-63. My thanks to Clive Perdue and the Editorial Board of *Encrages* for permission to reproduce parts of that article here. Thanks too to Elaine Andersen, Sue Foster, Mike Sharwood Smith and Jurgen Weissenborn for helpful criticism and to Erik Schils for statistical help. This version has been published in E. Kellerman and M. Sharwood Smith (eds.), 1986 *Crosslinguistic Influence in Second Language Acquisition*. Oxford. Pergamon

2. A not unrelated viewpoint is to be found in Day's study of fluency (Day, 1979:81):

> While it might be more 'ecologically valid' to have subjects just talk freely, such an approach has many inherent methodological difficulties. However, given the considerable amount of work needed to analyse the well-constrained data reported here, an exhaustive study of fluency might prove to be more exhausting than enlightening.

3. Fifteen out of the seventeen senses studied could in fact be translated by *break*.

4. Caramazza and Grober (1976) have shown that differences in grammatical category among senses do not affect similarity judgements. However, Kellerman (1982) has shown that they do affect transferability judgements.

5. The data were derived from the frequencies with which the senses were sorted together, using a technique developed and described by Miller (1969).

6. However, there is a strong correlation between derived similarity orderings and transferability orderings if the senses of *breken* are divided into concrete and abstract items. This does not rule out a frequency effect, though, since the *break* experiment deals with ordinal data, while the current experiment deals with actual interval data.

7 Caramazza and Grober's (1976) work on *line* is one exception since they (a) worked with polysemy, and (b) also included senses that function as verbs, i e. *to line a pocket*

8. The term 'radiate' is taken by Ullmann from Darmesteter (1946).

9 For an extensive study of body-part metaphors, see de Witte (1948).

10. By means of the MINUIT program (James and Roos, 1975)

11. Details of the statistical treatment of the data can be found in Kellerman (1980)

12. Work done by the author in association with Dianne Kramer shows that shape is more prominent for *electronic* than for e.g *potato, dice, or needle*

13. Nevertheless, studies of subjective frequency have shown very good correlations between these and objective frequency counts In fact it has even been suggested that the former are to be preferred since they do not suffer from the sampling bias of the latter (Carroll, 1971)

14. For instance, for *eye*, inter-subject agreement for similarity and frequency yields values for W (Kendall's Coefficient of Concordance) of 0 653, $p < 0.001$ and 0.711, $p < 0.001$ respectively.

APPENDIX A

List of senses used in *head* experiment

1. a head of steam
2. the head of a boil
3. the head on a poppy
4. the head on a glass of beer
5. the head of a nail
6. the head of the table
7. the head of a golf club
8. the head of a piece of paper

Note: Where a subject could not adequately define a sense in English, his judgements for that sense were discounted in both the Similarity, Frequency and Transferability tests.

APPENDIX B

Note: X^2 is calculated according to the formula presented in Spitz (1961). This formula was developed to deal with the problem of low frequencies in X^2 calculations.

L2 French Group (higher proficiency) N = 23

	O	E	O	E	X^2
steam : boil	17	14.738	1	3.261	3.331
steam : poppy	19	19.248	1	0.752	0.077
steam : beer	16	19.168	4	0.832	6.781
steam : nail	19	19.840	1	0.160	3.665
steam : table	17	17.810	3	2.190	1.888
steam : club	19	17.914	1	2.086	0.766
steam : paper	16	16.753	4	3.247	0.197
boil : poppy	17	16.997	3	3.003	0.000
boil : beer	8	17.557	13	3.443	21.967
boil : nail	15	19.294	5	0.706	12.023
boil : table	5	12.855	15	7.145	12.806
boil : club	8	13.106	12	6.894	5.404
boil : paper	5	10.663	15	9.338	6.645
poppy : beer	3	10.901	20	12.099	12.363
poppy : nail	10	19.053	13	3.947	18.099
poppy : table	1	5.547	22	17.452	6.763
poppy : club	5	5.783	18	17.217	0.146
poppy : paper	1	3.861	22	19.139	3.428
beer : nail	22	19.383	1	3.618	3.001
beer : table	12	5.999	11	17.001	7.061
beer : club	17	5.976	5	16.125	23.836
beer : paper	11	4.209	12	18.791	10.371
nail : table	1	1.359	21	20.641	0.111
nail : club	2	1.496	21	21.504	0.165
nail : paper	1	0.922	22	22.078	0.007
table : club	17	11.817	6	11.183	4.893
table : paper	7	8.930	16	14.070	0 704
club : paper	6	8.631	17	14.369	0.945

L2 French Group (lower proficiency) N = 48

	O	E	O	E	X^2
steam : boil	36	32.752	4	7.248	2.053
steam : poppy	35	37.533	4	1.468	3.128
steam : beer	36	38.336	4	1.664	2.490
steam : nail	38	39.679	2	0.321	4.032
steam : table	31	35.619	9	4.381	4.348
steam : club	37	35.829	3	4.171	0.403
steam : paper	31	33.507	9	6.494	1.053
boil : poppy	38	39.093	8	6.907	0.195
boil : beer	23	40.130	25	7.870	32.196
boil : nail	42	46.305	6	1.695	6.972
boil : table	14	30.852	34	17.148	24.421
boil : club	32	31.455	16	16.545	0.028
boil : paper	13	25.590	35	22.410	13.600
poppy : beer	10	21.801	36	24.199	13.011
poppy : nail	21	38.106	25	7.894	32.613
poppy : table	10	11.096	36	34.904	0.146
poppy : club	12	11.567	34	34.933	0.197
poppy : paper	9	7.723	37	38.277	0.243
beer : nail	40	40.451	8	7.550	0.029
beer : table	17	12.519	31	35.481	2.032
beer : club	29	12.221	16	32.779	27.168
beer : paper	17	8.783	31	39.218	7.874
nail : table	4	2.966	44	45.034	0.349
nail : club	11	3.123	37	44.877	13.418
nail : paper	5	1.926	43	46.074	3.602
table : club	36	23.120	9	21.880	15.892
table : paper	27	18.636	21	29.364	5.940
club : paper	13	18.012	35	29.988	2.340

L2 German Group N = 18

	O	E	O	E	X^2
steam : boil	16	13.920	1	3.080	2.207
steam : poppy	14	14.436	1	0.564	0.287
steam : beer	15	15.334	1	0.666	0.152
steam : nail	17	16.864	0	0.136	0.211
steam : table	13	15.139	4	1.862	2.156
steam : club	16	15.227	1	1.773	0.439
steam : paper	12	14.241	5	2.759	1.833
boil : poppy	13	12.747	2	2.253	0.035
boil : beer	6	14.212	11	2.788	19.848
boil : nail	15	16.340	2	0.600	2.249
boil : table	2	10.928	15	6.072	20.338
boil : club	10	11.140	7	5.860	0.330
boil : paper	3	9.063	14	7.937	9.257
poppy : beer	4	7.582	12	8.418	3.393
poppy : nail	8	13.254	8	2.746	9.031
poppy : table	2	3.860	14	12.141	1.359
poppy : club	1	4.022	15	11.978	3.966
poppy : paper	1	2.686	15	13.314	1.601
beer : nail	15	15.169	3	2.831	0.011
beer : table	3	4.694	15	13.306	0.909
beer : club	11	4.889	7	13.111	9.054
beer : paper	6	3.294	12	14.706	2.316
nail : table	0	1.112	18	16.888	1.814
nail : club	5	1.172	13	16.828	7.797
nail : paper	0	0.722	18	17.278	1.078
table : club	15	9.248	3	8.752	8.085
table : paper	10	6.989	8	11.011	2.054
club : paper	2	6.378	15	10.622	5.715

7 THE IMPERFECT CONDITIONAL[1]

7 0 Introduction

The Dutch have a reputation for being good at speaking English Their prowess in this regard cannot simply be attributed to the presence of English in the school curriculum For one thing, Dutch and English are typologically close for another, there is also considerable exposure to English through the media English and American productions feature regularly on Dutch TV and in the cinema and are never dubbed, while English-medium radio and television are becoming widely available English-language books sell so well that the British publishing trade apparently sees the Netherlands as simply another part of the home market The Dutch success in learning English also reflects an attitude fully commensurate with the commercial importance of the nation and the insignificance of Dutch as a world language Furthermore, the Dutch language, unlike French, does not have its official band of zealous guardians, it is a language that borrows happily, particularly from English

Despite the high level of linguistic achievement, it is obvious to any experienced observer that the majority of Dutch speakers remain unmistakably Dutch in their command of English This is not merely a matter of pronunciation, but also of 'accent' in other linguistic domains (e g Scarcella, 1983) These particular 'accents' take the form of characteristic features which are said to have fossilised (Selinker, 1972) Fossilisation occurs when learners stop learning, having arrived at a point where their language consistently deviates from native speaker norms, or when they regress to stages of development they had supposedly left behind It is a phenomenon whose existence carries considerable theoretical import, since its absence in child language development constitutes one of the cornerstones of the argument that the processes of first and second language acquisition are distinct

Although fossilisation has been frequently discussed in the literature, very little is known about its workings Biological explanations relating to changes in the brain ('the critical period hypothesis') have been put forward to account for the failure of

learners to achieve native-like proficiency, and so have psychosocial ones (see e g Klein, 1986 151 for discussion) However, such explanations are concerned with understanding the causes of fossilisation, they do not lead to predictions about what linguistic features of the interlanguage are candidates for fossilisation (for this point, with slightly different emphasis, see Selinker, 1974 42f) There has been virtually no discussion as to why certain 'accents' may come to typify a whole community of language learners irrespective of differing proficiency levels within that community An understanding of fossilisation may only come from a detailed analysis of such 'accents'

The existence of fossilisation presents a further challenge in environments in which formal language training takes place At university level in the Netherlands, a great deal of attention is paid to the appropriate morphology of English if-then conditionals, and it is well described in various grammars Yet explicit teaching does not seem to chalk up much success Dutch learners go on using would + infinitive (instead of a past tense) in the if-clause, producing such sentences as *If I would be able to live all over again, I would be a gardener* [2] Why then are Dutch learners even at this advanced level still immune to pedagogical blandishments? The causes of errors that persist at this level of general achievement deserve serious investigation

One might want to argue that the source of the learner's difficulty with conditionals is the native language, since Dutch in fact permits a would-equivalent in the conditional subclause However, this in itself is not an explanation for fossilisation Nor does it explain why Dutch should continue to influence the learner's language in this area of the grammar when its influence is more easily overcome in so many other areas where Dutch and English contrast All one could reasonably say about the L1 is that it could be the formal source of the learner's difficulties Clearly an explanation will have to be sought elsewhere

This chapter is an elaboration of the possible causes of the difficulty that even advanced learners have with the morphology of English conditionals Instead of putting forward a straightforward structural explanation for the problem, I shall propose that the

errors come about as a result of Dutch learners of English[3] a) reacting against ambiguity in the verb forms of conditional sentences, and b) creating morphological symmetry in verb forms of the two clauses that make up these sentences. Both these sentences may be found in a variety of language situations However, I shall also suggest that in the case of Dutch learners of English, these tendencies are inextricably linked to crosslinguistic influence.

The structure of the chapter is as follows: Section 7 1 provides brief descriptions of hypothetical conditionals in English and Dutch Section 7.2 describes the errors made by Dutch learners and reports briefly on experimental data from L1 Dutch and L2 English. I will then show in 7 3 that these same forms are also produced by learners with different language backgrounds, appear as variants in languages where they are sometimes stigmatised in standard descriptions, and are the accepted forms in others I will also present evidence that disambiguation can play a role in determining the structure of Dutch conditionals I will conclude that the avoidance of ambiguity and the promotion of morphological symmetry are primarily what cause the error and contribute to its fossilisation.

7.1 Hypothetical conditionals in English and Dutch

This section presents a brief sketch of English and Dutch hypothetical conditionals The former are well described in the literature (see e.g Quirk et al., 1985 and references therein), but Dutch conditionals have received only the most cursory attention (for a notable exception, see Nieuwint, 1984). It is not the aim of this section to remedy the current lack of an adequate description of Dutch conditionals, conditionals being complex structures on a number of levels. Rather I shall offer a characterisation of their structural properties, followed by some speculation about the meaning differences entailed by different structure types

I shall assume that a conditional sentence[4] consists of two clauses: the first, introduced typically by *if*, will, following traditional practice, be called the *protasis*. The protasis states the condition on which the second clause is contingent This second clause, again according to tradition, will be called the *apodosis*.

It optionally contains the correlative conjunction *then* [5] I will not in this section be considering other devices for indicating the contingency between clauses such as inversion of subject and tensed verb in the protasis, the use of other subordinating conjunctions such as *unless*, or co-ordination and juxtaposition of clauses

7 1 1 English conditionals

Comrie (1986 88-89) claims that languages organise conditionals along a hypotheticality continuum, and that different languages cut up that continuum in different ways, allowing speakers to express different degrees of hypotheticality by purely grammatical means Hypotheticality is defined by Comrie as follows

> the degree of probability of realisation of the situations referred to in the conditional, and more especially in the protasis .. a factual sentence would represent the lowest degree of hypotheticality, while a counterfactual clause would represent the highest degree

Conditionals, Comrie argues, say nothing about whether the propositions in either clause are factual or not, although it may be possible to infer their truth from other knowledge sources This means that what are traditionally called *open* or *real* conditionals (e g Leech, 1971 110) have, strictly speaking, very low hypotheticality since the speaker intimates that the probability of their fulfilment is relatively high Thus if someone says *It s nice out*, he presents this statement as a fact, and it has zero hypotheticality. On the other hand, *If it s nice out, we ll go to the park* does not state that either proposition is factual, even if the speaker believes that it actually is nice out, e g

(1) A. Is it nice out at the moment?
B· Yes
A Well, if it's nice out, we'll go to the park

Note that A's second remark can only be appropriate if A's knowledge of local weather conditions derives entirely from hearsay, i e B's reply (Akatsuka, 1986 340) If A is normally sighted and co-operative, he could not say *If it s nice out, we ll go to the park* while looking out of the window at the sunlit street below

Clearly, these conditionals do bear a degree of hypotheticality. However, backshifted tenses (i.e. a past tense to refer to non-past time and the pluperfect to refer to past time) in protasis and apodosis would indicate a lower probability of realisation, i.e. higher hypotheticality:

(2) If it was nice out, we would go to the park

Consequently, since we are dealing in known facts ('it's nice out') such a sentence would be decidedly odd if placed in the preceding interchange, because of its counterfactual reading:

(3) A: Is it nice out?
B: Yes
A: !Well, if it was nice out, we would go to the park

However, as Comrie (1986:89) amongst others points out, it would be wrong to assume that conditionals with backshifted tenses are necessarily to be interpreted as counterfactual, as the next example shows:

(4) A: Will you buy me a beer?
B: If you gave me a kiss, I'd buy you a beer

It is true that B might have said "If you give me a kiss, I'll buy you a beer" (Comrie 1986:90), but the point is that both backshifted and non-backshifted verb forms are possible, unlike in the preceding dialogue about the weather. The difference in the two sentences resides in the greater tentativeness of *If you gave me a kiss, I'd buy you a beer* (cf. James, 1982:391). The difference in interpretation as to the degree of hypotheticality between *If it was nice out* and *If you gave me a kiss* can only come about as a result of pragmatic considerations and not grammatical ones. The clearest cases of non-counterfactual readings for conditional sentences with backshifted tenses is when they occur in conjunction with a future time adjunct, e.g.

(5) If it rained tomorrow, we'd just stay home

Comrie's next example concerns what might seem an even stronger candidate for counterfactual status, a conditional sentence with a

pluperfect in the protasis and would + perfect infinitive in the apodosis. *If it had been nice out, we would have gone to the park* implies that the weather was unpleasant and we did not go to the park. However, Comrie again shows that counterfactuality is really an inference and not part of the grammatical meaning of the conditional.

(6) If the butler had done it, we would have found just the clues that we did in fact find.

This sentence leaves open the possibility that the butler really did do it (see also Davies, 1979). That past time conditionals of this kind are usually interpreted as counterfactual is presumably because we generally have more certainty about situations that may or may not have happened than we have about those yet to occur. Like other languages such as French and German, then, English conditionals grammatically distinguish only two degrees of hypotheticality, low and high. There is no specifically morphological means of expressing counterfactuality in such sentences (cf James, 1982.377-78).

We have seen that conditionals displaying high hypotheticality employ backshifted tenses. The rules for forming such conditionals in English are these:

- the protasis requires a finite verb with backshifted tense (preterite in non-past conditionals, pluperfect in past conditionals) or could, might and should + (perfect) infinitive, but not would [6]

- the apodosis requires a periphrastic conditional (would + (perfect) infinitive (though see section 7.3 below). Other auxiliaries are possible, but since would is the most common modal verb in the apodosis (Quirk et al., 1985.1010), they will not be referred to further.

Comrie's claim that conditional structures can be placed along a continuum of hypotheticality obviates the need to make 'the contorted and often empty formulations attempting to distinguish between real and hypothetical conditionals' (Comrie, 1986.88).

Accordingly, I shall from now on be dealing only with those conditionals whose backshifted tenses explicitly mark their high hypotheticality. Such conditionals will be known by the abbreviation HC (for 'highly hypothetical conditional').

7.1.2 Dutch conditionals

As suggested above, the organisation of the Dutch conditional system is more problematic than its English counterpart. Dutch conditionals of low hypotheticality are similar to English ones in using non-backshifted tenses. The difficulties start with the high hypothetical conditionals (HCs), where, on the face of it, there seems to be a number of choices in the morphological structure of the verb phrases of the two clauses.

In Dutch HCs with present or indefinite time reference (equivalent to *if it was nice out now/more often*) the protasis and the apodosis may be respectively marked either by a preterite or a periphrastic conditional (with what is formally a past tense modal equivalent of English *would*, *zou(den)*). That is to say, there are theoretically four possible structure types in Dutch HCs, all grammatical, as the following examples show:

(7) a. Als je dat zou doen, zou ik je zo de trap afschoppen
b. Als je dat deed, zou ik je zo de trap afschoppen
c. Als je dat deed, schopte ik je zo de trap af
d. Als je dat zou doen, schopte ik je zo de trap af
(If you did that, I would kick you downstairs)

Similarly, with past time reference, either the pluperfect or the perfect conditional may be used in either clause:

(8) a. Als je dat gedaan zou hebben, zou ik je zo de trap hebben afgeschopt
b. Als je dat gedaan had, zou ik je zo de trap hebben afgeschopt
c. Als je dat gedaan had, had ik je zo de trap afgeschopt

d. Als je dat gedaan zou hebben, had ik je zo de trap afgeschopt
(If you had done that, I would have kicked you downstairs)

In conditional sentences with explicit future reference (*If it rained tomorrow, we would go to the seaside*), the preterite in either clause is usually considered unacceptable

(9) a. Als het morgen zou regenen, zou de wedstrijd afgelast worden
b. *Als het morgen regende ...

This difference between conditionals with future and non-future reference in Dutch seems consistent with the observation by James (1982:378), who notes that past tenses are more common in non-future HCs than in future ones crosslinguistically In this respect, English and e.g French are exceptional, then, for requiring past indicative tenses with HCs with future reference For convenience, I shall code the grammatical structures of the clauses of a HC as follows·

+ signifies that the auxiliary verb *would*, or its equivalent in Dutch, *zou(den)*, is present in the protasis and/or apodosis, followed by the infinitive of a lexical verb, e g **if it would rain/ *If it would have rained: als het zou regenen/als het zou hebben geregend* (periphrastic conditional tense)

- signifies that *would* or its Dutch equivalent is absent, the finite verb being in a past tense form, such as preterite or pluperfect, e.g. *if it rained/if it had rained: als het regende/als het had geregend* (past tense)

HC sentence structure will always be given as if the order were 1) protasis, 2) apodosis, the clauses being separated by a slash, e g. +/+, -/+ etc. even where the order of clauses is reversed. Therefore both *If you gave me a kiss, I'd buy you a beer* and *I'd buy you a beer if you gave me a kiss* are coded as -/+

Thus in (7) and (8) above a. will be coded +/+, b -/+, c. -/-, and d +/-. Only b. is generally correct in English

Dutch grammars have had very little to say about the distribution and meaning of these structures, apart from noting that all four occur (Van Es and Van Caspel, 1975 191-94) Geerts, Haeserijn, de Rooij and v d Toorn (1984 468) maintain that the choice among the various structures in (7) and (8) is essentially stylistic The Nijmegen corpus of HCs being collected by Herman Wekker and myself shows that notion to be quite incorrect (see below) The one writer who has studied the significance of the formal diversity of Dutch HC morphology is Nieuwint (1984) His arguments are subtle and difficult to test, since they depend critically on the reader's acceptance of Nieuwint's intuitions about Dutch Accordingly, the description that follows is a distillation of Nieuwint's ideas coupled with our own observations on the Nijmegen corpus

It will be assumed that the principal difference between Dutch and English HCs is that Dutch does have the capacity to mark counterfactuality grammatically Specifically, in Dutch, speaker evaluation of the degree of hypotheticality may be expressed by a binary tense choice The use of a past tense -/ (preterite or pluperfect, according to time frame), in the protasis signifies the assertion on the part of the speaker that the preposition in the protasis is counterfactual (which is why the preterite cannot co-occur with a future time adjunct) This assertion may be even further underlined by the use of a past tense in the apodosis as well, which is then not only counterfactual by implication, but by grammatical marking (which is why past tenses are most common in past time HCs, where one can be most certain about what did or did not happen)

The use of the periphrastic conditional, on the other hand, only asserts hypotheticality, just as it does in English, counterfactuality is not part of the meaning of the periphrastic conditional tenses, though it may be interpreted by the listener as such in the appropriate context The examples in (7) and (8) above are arranged in this putative order of hypotheticality, (though we must exclude +/- structures for the moment) from the hypothetical +/+ (a) to counterfactual -/+ and doubly counterfactual -/- (b and c)

Periphrastic conditional HCs may be termed the unmarked or 'default' HC structures in Dutch That they can be used to refer to the same segment of the hypothetical ity continuum in their meanings as the putative counterfactuals may be seen from the observation that there is probably no Dutch HC with a past tense in either clause where that past tense cannot be rewritten by a periphrastic conditional (with one very minor exception) That is to say that any HC of the structure -/+, -/- or +/- may be reformulated as +/+ without loss of acceptability It is certainly not the case that any +/+ can be written as -/+, -/- or +/- The one exception to this 'rewrite rule' is als ik jou was, ("if I were you") where *Als ik jou zou zijn is unacceptable Although this expression really has idiomatic status, it can be assumed that the preterite is required here because of the physical difficulty involved in being someone else

As an example of this directionality in the substitutability of a structure by another, there seems, as I noted earlier, to be general agreement that in non-past time frames it is unacceptable to have a preterite in the protasis with a future time adjunct, viz

(10) *Als het morgen regende, zou de wedstrijd afgelast worden
(If it rained tomorrow, the match would be cancelled)

presumably because no-one can be so omniscient that they can rule out the prospect of rain Instead it would be necessary to have the periphrastic conditional in the protasis

(11) Als het morgen zou regenen,

When the time frame is indefinite, however, the speaker has a choice Thus written an indefinite time adjunct like vaker (more often'), either +/+ or -/+(-) is possible

(12) Als het vaker zou regenen/regende, werd het gras groener/zou het gras groener worden
(If it rained more often, the grass would get greener)

If past tenses seem to be less than acceptable in what are clearly potential contexts, the converse is not true The

conditional is perfectly acceptable in contexts which are irrealis, though presumably in the following example, the speaker is more concerned to fantasise than to assert the inability of houses to speak:

(13) Als dit huis zou kunnen spreken, wat voor verhalen zou het niet kunnen vertellen!
(If this house could speak, what stories it could tell!)

The same is true in past time frames (although the result is a little inelegant with its piled-up auxiliaries):

(14) Als dit huis zou hebben kunnen spreken, wat voor verhalen zou het niet hebben kunnen vertellen!
(If this house could have spoken, what stories it could have told!)

An example quoted by Dierikx (1985:50) may help to illustrate the intimate link between past tenses and counterfactual meaning, even in non-past contexts:

(15) "Dat wil niet zeggen dat ik een beest zou wezen. Er zijn genoeg mensen die me willen helpen, die me mogen. Als ik zo'n rotvent was (-PAST), hielpen (-PAST) ze me niet." Nou het is niet zo dat Bruessing een jongen is die op hulp zit te wachten.

("That does not mean that I am a brute. There are enough people who want to help me, who like me. If I was that sort of bastard, they wouldn t help me." Now it's not the case that Bruessing is the sort of bloke who sits around and waiting to be helped.)

Here 'Bruessing' has said quite explicitly that he is not a bastard, and that people are actually willing to help him. Thus both protasis and apodosis are counterfactual. -/+ and +/+ structures would have been equally acceptable, but the speaker's explicit commitment to the falsity of the propositions would have become progressively weaker, if still recoverable from the surrounding context.

This leaves us with the +/- structures It must be said that they are problematic, since in the present framework the protasis is hypothetical and the apodosis counterfactual Nieuwint (1984) claims that such structures mean that the speaker makes a categoric statement about what would happen at some point in time other than the present (apodosis) in a given situation which may or may not occur (protasis) It seems to be the case that these structures are often interpreted as overt promises or threats

(16) Als je dat zou doen, schopte ik je zo de trap af

or

(17) Als je beter zou zingen, verlengde ik je contract

The English equivalents would be sentences like

(18) Do that and I'll kick you downstairs

and

(19) Sing better and I'll prolong your contract

That +/- seems to be restricted to such cases is supported by the awkwardness of such sentences as

(20) ?Als je dat zou doen, speet me dat erg/vond ik
je een naarling

English equivalents also sound strange

(21) Do that and I'll be sorry/and I'll consider you
a fool

However, there are so few examples of +/- sentences in our corpus that it is difficult to check this characterisation against data The reader is referred to Nieuwint (1984) for discussion

This characterisation of Dutch HCs leads to a number of testable predictions about their distribution The first is that -/ protases should be commoner in past time frames than +/- The converse should also hold +/- protases should be more common in non-past time frames than -/ ones The third prediction is that in non-past time frames, / protases referring to the present or indefinite time

should be more frequent than those referring to the future

Stable linguistic judgements on the acceptability of HCs are very difficult to elicit from native speakers. Clear intuitions about what is or is not acceptable seem the exception rather than the rule Consequently the appropriate methodology for testing these predictions is to study HCs as they are actually used in non-experimental contexts, i.e in ordinary language use (cf. Lavandera, 1975). This is what Herman Wekker and I are doing at the University of Nijmegen. To date the Nijmegen corpus contains about 700 Dutch HC sentences collected from principally written sources accompanied by the contexts in which they appear. Table 1 gives the distribution of structure type by time frame in this corpus:

	Structure								
	+/+	-/+	-/-	+/=	+/x	-/x	x/+	x/-	Total
TIME FRAME									
NON-PAST	155	88	13	15	70	23	36	1	104
PAST	22	66	110	9	1	0	3	0	211
MIXED	7	9	5	4	1	2	0	1	29
									641

Table 1: Distribution of structure types, NHCC.
NB. X absent clause or verb form not realized by either past tense of lexical verb or conditional. In non-past HCs, many of these are present tenses
Mixed: Both non-past and past time frames used, e.g. "If I had seen her, I wouldn't be sitting here now"

If we exclude +/- structures from consideration (their frequency is small in any case), we see that in non-past time frames, there are 225 +/ protases (56%) and 126 -/ protases (30.8%). In past time frames, there are 23 +/ protases (10.9%) and 176 -/ protases (83.3%). Thus the first two predictions are confirmed The third

prediction has yet to be tested However, a quick perusal of the 64 -/ HCs in Dierikx (1985) suggests that this prediction may well be correct

Furthermore, Table 1 makes it quite clear that although all four HC structures are supposed to be grammatical, there are considerable differences in their distribution which cannot be attributed to stylistic choice +/+ is almost exclusively non-past, and -/- almost exclusively past +/- is rare Only the -/+ structure appears frequently in both time frames [7]

In summary, the principal differences between English and Dutch HCs are that Dutch seems able to distinguish grammatically the counterfactual in addition to the hypothetical This English cannot do A prediction about learner behaviour in English based solely on structural differences between the two language would anticipate that Dutch learners would overuse incorrect +/ protases in non-past HCs with future reference, and incidentally produce correct -/ protases in past and non-past HCs whenever they wished to mark counterfactuality grammatically as they would in Dutch As far as the English apodosis is concerned, / would predominate in past HCs and /+ in non-past HCs Whether these predictions have any substance is the topic of the next section

7 2 A comparison of performance in L1 and L2

7 2 1 Errors made by Dutch learners of English

Now that we have briefly described HCs in both English and Dutch, we must consider the errors that Dutch learners of English make As has already been pointed out in the introduction, advanced Dutch learners of English frequently use would in the protasis of HCs, as in (22)

(22) a *If it would rain, they would cancel the concert in Damrosch Park +/+

b *If it would have rained, they would have cancelled the concert in Damrosch Park +/+

Such use constitutes an error in that English HCs do not normally allow the modal auxiliary 'll to occur in the protasis Less

frequently, one finds errors like (23) a and b·

(23) a. *If it rained tomorrow, they cancelled the concert in Damrosch Park -/-

b. *If it had rained yesterday, they had cancelled the concert in Damrosch Park -/-

The rule violated here is that English HCs do require a construction with would (or some other past tense modal auxiliary) in the apodosis (matrix clause). Double mistakes also occur, though they seem rare:

(24) a. *If it would rain tomorrow, they cancelled the concert in Damrosch Park +/-

b. *If it would have rained yesterday, they had cancelled the concert in Damrosch Park +/-

The correct versions of (22)-(24) would thus be 25) a and b.

(25) a. If it rained tomorrow, they would cancel the concert in Damrosch Park -/+

b. If it had rained yesterday, they would have cancelled the concert in Damrosch Park -/+

7.2.2 Two experiments

Wekker, Kellerman and Hermans (1982) investigated performance on non-past and past hypothetical conditional sentences in Dutch and English under experimental conditions, using Dutch learners of English as their own controls. That is to say, subjects performed the same task in both languages, thus permitting comparison both within and across individuals Wekker et al also collected data using two slightly different elicitation instruments. The first required the subjects to complete paragraphs with a contextually appropriate conditional sentence.

The second elicitation format also required subjects to complete a paragraph, this time by choosing from four alternative contextually appropriate HCs, each with the same content and representing one of the permissible Dutch structures in (7) and (8) above The first instrument (Experiment 1) was thus more 'natural',

while the second (Experiment 2) invited subjects to consider the structure of the response quite explicitly. Subjects were drawn from 1st, 2nd and 3rd year students at the University of Nijmegen, generally considered to be advanced after between 7 and 9 years of English instruction.

The distribution of L2 HC structures obtained in the two experiments is shown in Tables 2 and 3:

English structure	NON-PAST 1st year	2nd year	3rd year
+/+	56 (75%)	59 (81%)	22 (47%)
-/+	19 (25%)	14 (19%)	25 (53%)
+/-	0	0	0
-/-	0	0	0
	PAST		
	1st year	2nd year	3rd year
+/+	20 (24%)	8 (10%)	7 (12%)
-/+	60 (73%)	73 (88%)	50 (85%)
+/-	1 (1%)	0	0
-/-	2 (2%)	2 (2%)	2 (3%)

Table 2: Experiment 1 - Relationships between non-past, past and preference for English HC structures per proficiency group

English structure	NON-PAST 1st year	2nd year	3rd year
+/+	77 (56%)	55 (45%)	22 (25%)
-/+	44 (31%)	56 (46%)	61 (70%)
+/-	17 (12%)	8 (7%)	3 (3%)
-/-	2 (1%)	3 (2%)	2 (2%)
	PAST		
+/+	57 (41%)	22 (18%)	11 (13%)
-/+	68 (49%)	97 (79%)	75 (85%)
+/-	6 (4%)	1 (1%)	1 (1%)
-/-	9 (6%)	2 (2%)	1 (1%)

Table 3: Experiment 2 - Relationships between non-past, past and preference for English HC-structure per proficiency group

These figures show that where focus is not explicitly on form (Experiment 1), even 3rd year students have not mastered non-past HC syntax, although all three groups score highly in past HCs All in all, performance in Experiment 2 is better for non-past HCs, while 1st year subjects are somewhat worse for past HCs than they were in Experiment 1.

At first sight, Tables 2 and 3 might be seen to support the idea that Dutch is the source of the distribution of structures in English, since these frequencies bear some resemblance to the distribution of Dutch structures in Table 1 in the previous section, at least as far as the choice of verb-form in the protasis is concerned Thus, it might be argued, the reason why Dutch learners put woulds into their protases is that they transfer the periphrastic conditional distribution from Dutch Such an explanation still does not account for the tendency for this error to fossilise, though It says merely that Dutch and L2 English are structurally similar. Nor does it account for the clear differences in distribution revealed in Tables 1, 2 and 3 for the -/- structure in Dutch and L2 English past HCs

As noted above, having subjects as their own controls permits us to compare Dutch and English responses per individual subject as well as per group. In fact, the application of a simple procedure we called shift analysis (Wekker et al , 1982) enabled us to point out that the similarities between Dutch and English performance were less apparent once one began to examine behaviour beyond the level of the group. Shift analysis compares performance in the two languages for each subject on the same stimulus and notes a) whether the same or different structures are chosen in both languages, and b) if different structures are chosen, what the direction of the change is (i e. towards the target English structure, or towards another Dutch-like structure)

The analysis reveals that 50% of the English responses were shifts and that shifting was overwhelmingly in the direction of either the correct -/+ structure or +/+ in both experiments. The percentage of shifts to +/+ contributed by each group is

proficiency-related, with 1st years providing the most (Table 4). The ratio of shifts to -/+ against shifts to +/+ shows another clear proficiency-related progression (Table 5). Taken together, Tables 4 and 5 show that increasing nominal proficiency goes hand-in-hand with more native-like performance on HC structure. However, Table 2 reveals that even 3rd years do not behave like native speakers. In sum, shift analysis revealed that subjects did not merely use the same structure in English as they did in Dutch. All groups showed at least some tendency to select +/+ as the English target, irrespective of their choice in Dutch. As a result, alternative explanations for the occurrence of this structure will have to be sought.

	NON-PAST		PAST	
	Expt. 1*	Expt. 2	Expt. 1	Expt. 2
1st year	-	.56	.61	.58
2nd year	-	.31	.18	.27
3rd year	-	.13	.22	.15

Table 4: Proportion of total shifts to +/+ contributed by each proficiency group.
*Not calculated as frequencies too small

	NON-PAST		PAST	
	Expt. 1*	Expt. 2	Expt. 1	Expt. 2
1st year	8	1.55	2.35	1.14
2nd year	0	4.50	11.10	3.08
3rd year	23	12.25	5.67	5.57

Table 5: Factors by which frequency of shifts to -/+ exceeds shifts to +/+/.
*These figures are based on very small +/+ frequencies (see Table 3).

7.3 An alternative explanation

7 3 1 Structural disambiguation

In this section I shall propose that the influence of the L1 in the acquisition of English hypothetical conditionals is a great deal more subtle than a simple structural comparison of Dutch and English conditionals might suggest I shall argue that the difficulty experienced by Dutch learners with these structures is related to their attempts to deal with polysemous verb morphology in English and Dutch HCs and with the subtle differences in meaning that exist between formally similar structures in the two languages The solution which Dutch learners come up with, double would constructions, seems consistent with solutions to a number of other acquisition problems discussed elsewhere in the literature Furthermore, it also resembles observable tendencies in the HCs of a number of standard and non-standard languages

Recall that Dutch HCs may use either modal past tenses (preterite and pluperfect) or periphrastic conditionals with zou(den) + infinitive in HCs We have seen that there are clear differences in distribution between the various structural possibilities permitted by Dutch, and I have claimed that these differences reflect grammatically encoded meaning distinctions in terms of the degree of speaker commitment to the realisability of the propositions in protasis and apodosis

I have assumed that in Dutch HCs, the preterite expresses an irrealis condition in present and indefinite time-frames, not only on the basis of contextual clues, but also because of its unacceptability in HCs with future time adjuncts But this is the modal meaning of the preterite, which tense form normally refers to real situations occuring in past time frames Modal and non-modal meanings of the preterite therefore differ along two dimensions (hypotheticality and time-frame) As James (1982 398) points out

> .. it is not normally the case that in order to learn where it is proper to use a past tense morpheme to indicate past time, what native speakers must do is memorise a list of specific constructions. But in the case where a past tense morpheme is being used to indicate the hypothetical, that is precisely what native speakers must do ... this means that the use of the morpheme in question ... to indicate past time and its use to indicate the hypothetical are not really parallel The former use if normally regular and productive; the latter use is typically irregular and idiosyncratic

Accordingly, I shall call the modal meaning of the past tense the *marked* meaning.

The pluperfect tense also has a normal reading (real, 'past-in-the past', Leech, 1971:42), and a marked, modal meaning (counterfactual, past). Unlike in the previous case, however, the semantic distance between the two meanings is principally along the hypotheticality dimension and is not as striking as in the case of the preterite.

Our data suggest that Dutch learners of English at advanced levels are unwilling to transfer the marked modal meanings of the Dutch past tenses to their formal English equivalents. If they did so, they would be imbuing the English past tenses with the same idiosyncratic meaning that they know is permitted in Dutch. This is a subtle form of crosslinguistic influence, in that the L1's role is to constrain the form that the developing interlanguage may take rather than to provide a structure for copying over into the L2

This reluctance to permit an equally marked structure in English is particularly understandable in the case of the preterite, because the mismatch between unmarked and marked meaning is greater here than in the case of the pluperfect. It is relevant to note that this reluctance to allow marked forms in L2 HCs may well be developmental, since in a replication of Wekker et al. (1982) with less proficient Dutch school-age subjects (Klein, 1982), structural transfer occurred much more frequently (particularly in the form of non-shifted -/- in past HCs) than it did in the data from advanced learners described above (Wekker et al , 1982)

Even though learners are unwilling to transfer the marked reading of the Dutch preterite and pluperfect to their English equivalents, they are of course still able to produce HCs, since Dutch provides the +/+ structure as an alternative. We have argued

earlier that this latter structure is the default structure in Dutch Once we accept that this is so, then we see that advanced Dutch learners of English transfer the default structure to English, whatever they do in Dutch

Thus Dutch learners of English sacrifice the grammatical distinction that Dutch makes between potentialis and irrealis conditionals in response to the structural ambiguity of the preterite. This sacrifice is not in fact particularly great, since as in other languages like English and French the degree of commitment by the speaker to the falsity of the proposition expressed in the protasis can also be inferred from the context in which the HC is embedded or from other knowledge sources (see the examples in section 7 2 above) Thus the Dutch learner can effectively avoid the preterite in English HCs, reserving the periphrastic conditional tense as the sole means of marking hypothetical meaning in HCs

Furthermore, this solution to the problem of how to form HCs in the L2 is, from a strictly communicative point of view, unlikely to engender negative feedback from native speakers of English (except of the metalinguistic sort in pedagogical environments), since the +/+ structure is unlikely to cause misunderstandings amongst native speakers This fact, the fact that there are subtle meaning differences between the Dutch and English uses of the marked modal forms, and the reluctance on the part of the advanced learners to assign marked status to the English preterite would seem to conspire to keep the English input opaque

7.3 1.1 Supporting evidence

The interpretation of the +/+ error made by Dutch learners thus rests on two assumptions a) learners will not use modal past tenses in English HCs because of their marked status, and b) Dutch provides an alternative structure which expresses hypotheticality and is indifferent towards the potentialis – irrealis distinction We must now look for evidence which supports this particular interpretation. There seems to be two sorts which are relevant The first derives from acquisitional studies where learners react against structural ambiguity The second derives from the

examination of HCs in various languages where similar tendencies are apparent in non-acquisitional settings

7.3.1 2 Evidence from first and second language acquisition

Dutch learners of English are not the only ones to make +/+ errors. Nemser (1974 57), for example, reports that many Hungarian learners 'overtly indicate contingency in both the protasis and apodosis of conditional sentences' by inserting would in both clauses. Hebrew-speaking learners make the same error (Levenston, 1970·214), and it has also been reported at least for Czech (Vladimir Mach, personal communication), Finnish (Kari Sajavaara, personal communication), German (frequent personal observation), Polish (Waldemar Marton, personal communication) and Serbo-Croatian learners.

In all these cases, there are good structural reasons why the error should occur, since hypotheticality is marked explicitly in both clauses in all the relevant L1s In Hungarian, for instance (Tompa, 1968.100-1), non-past HCs require the present conditional tense in both clauses, while past HCs require the perfect indicative plus a particle volna (historically the conditional form of the verb *to be*). Similarly, Hebrew and Polish require parallel marking in both clauses (viz. Hebrew *im hayita oved, hayita macliax*, 'if you worked, you would succeed', Polish *gdybyś pracował, odniósłbyś sukces* - 'if you worked/had worked, you would succeed/would have succeeded').

For our purposes, the most directly relevant study is the one reported by Trévise (1979) She found that her subjects, French first and second year university students of English, 1) had great difficulty in forming English HCs, 2) made +/+ errors when they did, and 3) in some cases altered correct -/+ HCs to incorrect +/+ in a subsequent editing phase. French is like English in not marking counterfactuality in HCs grammatically The protasis requires an imperfect or pluperfect indicative, depending on the time frame, and the apodosis a corresponding conditional or conditional perfect (though see below). Thus French learners are also faced with the problem of coping with the modal meaning of what is formally a past tense in French and how to express it in English.

From a contrastive point of view, it is much more difficult to explain away Trévise's findings by an appeal to a structural description of French, since the standard language does not permit a double conditional structure Instead, if a crosslinguistic explanation was insisted upon, one would have to invoke non-standard varieties of the language and show that it was these that influenced learners rather than the standard language. We return to this point below. In the literature of second language acquisition there is further evidence that learners will not transfer (or are reluctant to transfer) L1 features which they consider marked. This reluctance to resort to the L1 may be developmental, as I suggested above with respect to the Klein (1982) study, and may happen despite the nature of the target input. That is to say, even where the L2 and the L1 are equivalent, learners may behave as if they were not. (For a discussion of this phenomenon, see Kellerman, 1985a)

For instance, in studies of second language acquisition, Kellerman (e.g. 1982, 1986) showed that Dutch learners of English and German did not tolerate the same degree of polysemy in L2 lexical items as they knew existed in their L1 equivalents. Furthermore, the various meanings of the polysemous lexical items in Dutch were reallocated to other lexical structures in the L2 as a function of the perceived resemblance of those meanings to the prototypical (unmarked) meaning of the polysemous lexical items - the greater the distance from this prototype, the stronger the assumption by the learner of non-equivalence between the two languages, irrespective of the actual linguistic facts.

Similarly, Hoeks (1985) showed that Dutch learners translated polysemous Dutch nouns into English according to the semantic distance they perceived between them - the greater the distance, the more likely more than one translation equivalent would be sought, even if ultimately they could not find one. In the same vein, Ijaz (1985) found that learners of high proficiency from various linguistic backgrounds (principally German and Urdu) tended to transfer the prototypical meanings of L1 prepositions to their English equivalents, while less proficient ones assumed that L1 and L2 were semantically equivalent.

Gass and Ard (1984) studied Spanish and Japanese learners'

responses to tense usage in English, and found that they were more likely to accept (and produce) prototypical meanings of tenses than secondary ones, even where, as in the case of Spanish, L1 and L2 overlapped. In all these second language cases, learners often assumed non-equivalence between L1 and L2 when there frequently was such equivalence. In first language acquisition, Karmiloff-Smith (1979) has shown that French-speaking children reanalyse structures with two meanings they had previously used in adult-like fashion so as to give each meaning a unique linguistic form. In doing this, they create a novel, if plausible, form for one of the meanings, retaining the original form exclusively for the other meaning. Later, these same children recombine both meanings into the original single form, and the novel form disappears.

7.3.1.3 Supporting evidence from other languages

I have claimed above that the preponderance of the +/+ error in the English of Dutch learners comes about as a result of their unwillingness to assign modal meaning to past tense forms in the L2, since this is the marked meaning. In fact, although past tenses are widely used to express hypotheticality in the world's languages (James, 1982), we can also adduce evidence from a number of languages that the tendency to disambiguate verb morphology in HCs is not unique to Dutch learners of English. Let us begin with Dutch.

a) Clause order and verb morphology in Dutch

Despite the fact that Dutch allows past tenses of lexical verbs in both protasis and apodosis, native speakers often indicate a dislike of the past tense in an apodosis that has been preposed. Thus while

(26) Als je zoiets zou doen, schopte ik je de trap af

If you such-a-thing would do, I kicked-PRET you downstairs

(If you did such a thing, I'd kick you downstairs)

is, as we have seen, perfectly acceptable to native speakers, the

following is less so:

(27) ?Ik schopte je de trap af, als je zoiets zou doen
(cf. Nieuwint, 1984)

Instead there is an expressed preference for the periphrastic conditional in a preposed apodosis, viz.

(28) Ik zou je de trap afschoppen als je dat zou doen

A perusal of the Nijmegen HC corpus (c. 700 sentences) clearly supports native speaker intuitions on this point. The corpus shows that Dutch, like other languages, favours the order protasis-apodosis (Greenberg, 1963; Ford and Thompson, 1986; Comrie, 1986).[8] About 70% of HCs in our corpus have that order (450:192). However, when we analyse clause order by structure type (i.e. +/+, -/+, -/-, +/-), we find that, proportionately, the distribution of clause orders is not evenly spread.

Of the 386 HCs with /+ apodoses in our corpus, only c. 60% have the order protasis-apodosis (233:153), while of the 158 HCs with /- apodoses, as many as c. 84% have this order (133:25). This distribution is not sensitive to time frame. C. 59% of non-past (166-113) and c. 54% of past /+ apodoses (44:37) have this order; the comparable figures for /- apodoses are c. 90% (26:3) and c. 82% (97:22).[9]

Comrie (1986) notes that in the world's languages the protasis is more usually overtly marked as non-factual than the apodosis (i.e. via a subordinating conjunction like *if*), which might be thought to explain why protases tend to precede apodoses. However, Comrie points out that this cannot be the whole story, since it would lead to the prediction that in those (few) languages where only the apodosis is explicitly marked for non-factuality, it should more frequently precede the protasis than in cases where it is not so marked. There is apparently no evidence in favour of this hypothesis.

In fact, in a language like Mandarin, the protasis must precede the apodosis, whether either, both, or neither is marked for hypotheticality.[10] As Comrie and others point out, and as we have already seen above, in English it is quite possible to have

minatory/promissory conditionals without any explicit hypothetical marking, of the type *Laugh and the world laughs with you/Score tomorrow and we'll take you off the transfer list* where no-one would interpret the protasis as an injunction to act. Even if the change in verb forms was originally related to the avoidance of the momentary ambiguity between real and hypothetical meaning, it seems to have long been grammaticised (Comrie, 1986). Nevertheless it is difficult to see what else could be motivating the clear trend against preposed/- protases in our Dutch data if it is not the absence of specific grammatical 'priming' of the modal meaning of the verb.

b) Structural disambiguation in German HCs.

Similarly, there is evidence in German that verb forms that are structurally ambiguous in HCs will be avoided. In HCs, German may either use the subjunctive or a periphrastic conditional with *wurden* + infinitive. However, the subjunctive is avoided when this verb form is indistinguishable from the preterite.[11] In such cases the periphrastic conditional is preferred.

(29) a. Wenn es regnen wurde, . . (If it rain would)
b. ?Wenn es regnete, ... (If it rain-SUBJUNCTIVE)

However, if subjunctive and past tense are not formally identical, then either periphrastic conditional or subjunctive seem permissible:

(30) a. Wenn er kommen wurde, .. (If he come would)
b. Wenn er kame, .. (If he came-SUBJUNCTIVE)

Cf.

c. Wenn es regnete, kam er spat nach hause
(Whenever it rained-PRET, he could wome-PRET home late)

c) Evidence from French

As noted above, in standard French there is no structural correlate of +/+ in conditionals introduced by the subordinating conjunction *si*. Non-past HCs require the imperfect indicative in

the protasis and the conditional in the apodosis, while past HCs require the pluperfect and the conditional perfect in the two clauses: *Si j'avais le temps, je t'expliquerais/si j'avais eu le temps, je t aurais expliqué.* However, in non-literary, informal French, where protasis and apodosis are simply juxtaposed or co-ordinated without *si*, the conditional tense is obligatory in both clauses, and the order must be protasis-apodosis, as in informal spoken French:

(31) J'aurais le temps, je t'expliquerais (see also Posner, 1976)[12]

Furthermore, it has been observed that although clearly stigmatised (e.g. Grévisse, 1964.1081), the conditional tense does occur in *si*-protases in *le français populaire*. Interestingly in this respect, Claire Blanche-Benveniste (personal communication) has recorded that, in reading aloud, French children up to as late as ten years of age may spontaneously replace the imperfect or pluperfect tense of protases introduced by *si* with a conditional tense. Again we see that the standard language avoids past indicative tenses in protases where they are not preceded by a subordinator. That the conditional should appear in *si*-less clauses again suggests a response designed to obviate the structural ambiguity of past tense forms.

d) Evidence from English

While standard descriptions of both British and American English insist that modal past tenses (typically preterite and pluperfect) are required in the protases of HCs (except in one or two minor cases, e.g. where *would* is volitional), it is not difficult to find contemporary and historical evidence for its existence Non-standard usage is reputedly common in informal American speech and is occasionally detected in adult British speakers.

(32) I would have been much more happier if he wouldn't have head-butted me (Professional boxer, BBC Radio 4)

It should be pointed out, however, that the present author could not find one clear example of a *would* protasis in either the Brown or

LOB English corpora

However, examples of protases containing would or its Middle English equivalents may be attested as early as c. 1225 (Visser, 1963:1731):

(33) Her may me waiten for to slo, and yf he were brought of Liue. And mine children wolden thriue, Louerdings after me Of al Denemark mihten he be

(They would wait for me to stay; and if they were killed and my children would thrive, princes after me of all Denmark might they be)

They are also attested among young (American) children (Kuczaj and Daly, 1979), although this might merely reflect the input.

(34) If you would have eated all that turkey, your tummy would have kersploded (age 3.11)

A second non-standard variant in the protasis is common in both British and American English. This is intrusive have (Fillmore, 1985), combined with d in sentences like *If I d ve known you were coming, I d ve baked a cake*. In the spoken language, it does not seem stigmatised, though it is railed against by purists such as Fowler (1965).[13] In fact, Lambert (1983, cited by Fillmore, 1985) claims that the use of *intrusive have* in British English does not vary according to social class, and native speakers tend not to be aware of using it.

In American English it has long been recognised as very common (see Visser, 1973.2423 for sources). In England, examples may be found going back to c. 1400 (Visser, 1973:2425), where, contrary to current practice, would did not appear in the apodosis either.

(35) and (= if) I had natte have had that mony of William Barwell at that tyme, I had lost all my platte (1475-88)

Fillmore (1985) argues that'd have is not to be treated as a colloquial variant of the standard pluperfect, since one cannot say

(36) *At that time I hadn't have opened your letter

as a variant of

(37) At that time I hadn't opened your letter

or

(38) *She telephoned me as soon as she'd'ye finished eating dinner

for

(39) She telephoned me as soon as she'd finished eating dinner

Redundant have can only be used now in the protases of past HCs or other clauses expressing hypotheticality, e.g these contemporary (British) examples from Fillmore (1985) and elsewhere

(40) a. If I'd've known Renoir was this popular, I would never have come - Punch cartoon
b. I wish I hadn't've said that
c. By the time you'd've noticed it, it'd've been too late

Thus Fillmore suggests that all the evidence points to the fact that intrusive have is an explicit marker of counterfactuality (a view shared by Visser, 1973·2424); for reasons explored earlier, it would be more accurate to call it an explicit marker of hypotheticality in a past time frame.

In all the above cases where HC morphology has been discussed, there seems to be evidence for a process of grammatical disambiguation of past tenses While in established languages such tendencies cannot realistically be associated with ease-of-processing requirements, in the case of learners of English this is very likely to be the motivating force. We have seen that the same tendencies appear when learners are confronted with other structures with multiple meanings. The result of such disambiguation should be a structure with greater perceived isomorphism between form and meaning and a correspondingly smaller processing load for the learner. Since this tendency is discernible in a number of differing linguistic contexts, we may call it a

natural tendency

7 3 2 Morphological symmetry in protasis and apodosis

In the cases illustrated so far, there has been a tendency to reanalyse past tenses into other verb forms in HCs It will not have escaped notice that the resultant change in the protasis frequently leads to morphological symmetry in the verb in protasis and apodosis In fact there are many standard languages which require identical morphosyntactic constructions in the two clauses of a hypothetical conditional sentence [14] Furthermore, in addition to the English, French, German and Dutch examples discussed above, there are other non-standard variants of languages not normally having symmetry where symmetry is, or is becoming, the norm [15]Again we may speak of a natural tendency in languages

As Posner (1976) and others have noted, there is a strong tendency in many Romance varieties towards morphological symmetry in protasis and apodosis (as was the case in Classical Latin) Posner states that it is actually the standard versions of these languages (and Catalan) that constitute the exception (though Rumanian does have symmetry) Symmetry is simply a pervasive phenomenon in Romance Posner thus rejects crosslinguistic influence as the source of parallelism (in the form of *si* + *-rais/ -rais*) in Canadian French (attributed to American English), in North African French (Spanish), or in Belgian French (Flemish) Similarly, says Posner, there is no need to attribute intrusive *would* in New York City English to Yiddish

Now the appearance of morphological symmetry in HCs does not automatically entail the selection of one particular set of tenses rather than another (i e past tenses vs (periphrastic) conditionals or subjunctives) For instance, in addition to the conditional, some Italian dialects require their HCs to have two imperfect indicatives, and others imperfect subjunctives (Posner, 1976, Pountain, 1983, Harris, 1986) In any case, whatever the verb forms chosen, this preference for symmetry in the HCs of so many differing languages is striking For Haiman (1986 221), the semantic symmetry of protasis and apodosis is reflected in their morphological symmetry

Dutch and French learners of English show a similar trend towards verb-morphological symmetry. However, as will by now be obvious, a tendency towards symmetry alone is not sufficient 'explanation' for the appearance of symmetrical structures noted in their English. If it were, we could equally well expect to find symmetrical structures with past tenses, but such structures, common enough in Dutch, are very rare among advanced Dutch learners as we have seen. Thus, in the case of Dutch learners, the drive towards disambiguation takes precedence over the drive towards symmetry. The same could be said for French learners of English However, in the case of Dutch learners, the presence of a 'default' +/+ with double zouden in Dutch may have a catalytic effect on their English, in the case of French, this is less clear

That disambiguation leads to selection by the learner of a periphrastic conditional in English should not be surprising Like zouden and -rais, would is an explicit marker of hypotheticality. +/+ morphology is therefore simultaneously unambiguous and symmetrical, it is maximally semantically transparent (Slobin, 1977, 1980; Langacker, 1977, Naro, 1978, Kellerman, 1983, Andersen, 1984b, Seuren and Wekker, 1985)

This leaves us with one problem Why do Dutch learners on the whole find it easier to produce the desired English -/+ HC in past environments than in non-past environments? That is, why do they move from the highly frequent Dutch -/- to English -/+, rather than to +/+? After all, -/+ is neither maximally transparent nor morphologically symmetrical [16]

One possibility is that learners might feel less need to avoid the pluperfect in a past environment, because the modal meaning is closer to the non-modal meaning than is the case in non past HCs This might make the modal meaning of the English pluperfect more salient in the input, especially as, like Dutch, it will usually be interpreted as counterfactual. But this argument does not hold for the apodosis; the /+ apodosis is normal in the L2 English of advanced learners, while the /- apodosis is the most common form in Dutch

Perhaps the answer is really quite straightforward, these are advanced learners, and they do learn *something* about English HCs in

all their years of study It is the English protasis that is the locus of the acquisition problem, not the apodosis The English apodosis requires would (or some other modal), it is therefore semantically transparent whether past or non-past, a fact which makes it salient and learnable

7.4 Conclusion

In this chapter, I have proposed that Dutch learners of English produce double would structures such as *If I would be able to live all over again, I would be a gardener* in English HCs as a response to two tendencies The first tendency is to avoid transferring the modal meaning of Dutch past tenses to English past tenses even though there is positive evidence for this in the input Instead the modal meaning is reallocated to the explicitly hypothetical morpheme would, which is the equivalent of Dutch zouden There is also evidence that under certain conditions even standard languages will avoid structurally ambiguous verb morphology in HCs Similar attempts at disambiguation by learners are reported in other areas of the grammar in both the first and second language acquisition literature.

The outcome of the disambiguation process in many cases is a hypothetical conditional with symmetrical verb morphology in protasis and apodosis. This second tendency towards symmetry in HCs is to be found in the standard versions of many languages It is also found in non-standard variants of those languages which do not normally permit it Since learners of English from several language backgrounds also produce symmetrical HCs, these tendencies towards symmetry and towards structural disambiguation in learner language provide further evidence that interlanguages obey the same constraints as natural languages

In the case of Dutch learners we see that the tendencies to disambiguate past tenses and to produce symmetrical morphology conspire to produce a characteristic error While disambiguation of the past tense occurs in languages other than Dutch, and non Dutch learners of English, we should not ignore the fact that Dutch structure as perceived by the learner provides the environment in which these tendencies become apparent There is therefore an

interaction between natural tendencies and the native language

The result of the interaction is a semantically transparent HC grammar which is simpler than both L1 and L2 in not using modal past tenses Furthermore it is also simpler than Duth in not making a morphological distinction between hypothetical and counterfactual The reason why the +/+ error may fossilise may thus reside with the difficulty of overcoming these natural tendencies, however, the Dutch learner has also to discover that while English does have modal past tenses their use in hypothetical conditionals is subtly different from the way they are used in Dutch Such a suggestion, while entirely speculative at this stage and in no sense an explanation for fossilisation, has an advantage over contrastive accounts in its appeal to the workings of languages in general

NOTES TO CHAPTER SEVEN

1. This chapter is based on a presentation given at the Symposium on Systems Interaction in Bilingualism held at the City University of New York in July 1986. It is to appear in K. Hyltenstam and L. Obler (eds.), Bilingualism across the Lifespan: In Health and in Pathology. Thanks are due to Theo Bongaerts, Carlos Gussenhoven, Peter Jordens, Herman Wekker and Brian Wenk for comments on an earlier version.

2. Here are two examples. The first is taken from a recent book on computational linguistics:

> If in a logical calculus a means would have to be built to establish their truth, one would have to create something that itself would be a model of the contents of the sentence ...

The second is from a report:

> It would be unfortunate if our classification system would not enable us to break down the main types and investigate more superficial, formal, differences

3. And of French, for that matter

4. There are several types of conditionals. For discussion, see Pountain (1983), Traugott et al. (1986).

5. Preposing the apodosis in English and Dutch also entails the obligatory absence of the correlative conjunction, viz. *Then the concert would be cancelled if it rained.

6. There are of course exceptions to this statement. So-called 'volitional' would is an obvious example. See for instance Nieuwint (1986)

Instead of the past tense or the pluperfect of a lexical verb, English HCs may also allow a construction with was/were to or with should. These more formal and tentative types of HC (see Quirk et

al., 1985:1093-94) will not be considered here.

7. There may be some purely morphological factors affecting distribution which we have yet to investigate. In non-past time frames, the majority of verbs in -/ protases are highly frequent strong verbs with monosyllabic stems. In fact, 17 such verbs or their derivates accounted for c 95% of the protases in -/+ and -/- sentences (blijven, doen, gaan, hebben, komen, krijgen, kunnen, laten, liggen, moeten, mogen, rijden, staan, weten, worden, zeggen, zijn), there were only 5 regular ones (beschouwen, beweren, horen, leven, openen).

However, there are 58 different verbs in the protases of non-past +/+ HCs, including most of the above verbs, i e doen, gaan, hebben, komen, krijgen, kunnen, laten, moeten, mogen, staan, worden, zeggen and zijn Together these latter verbs account for c. 53% of all such +/ protases.

8. To be accurate, Greenberg, Ford and Thompson, and Comrie are dealing with conditional sentences of all types Ford and Thompson's study is actually about frequencies of if-conditionals. However, this is unlikely to have any direct bearing on clause order. Of the 490 written conditional sentences they collected, 377 (77%) had the order protasis-apodosis, of the 406 spoken conditionals, 82% had this order

9. Furthermore, when we consider 'extraposed' HCs (cf Pountain, 1983) where the apodosis contains a pronoun (usually subject or object of the clause) which refers to the whole protasis (e g. It would be nice if you could come), the normal clause order is usually reversed. In our corpus there are 99 such extraposed HCs (c. 15% of the total); c. 76% of these HCs show the order aposodis-protasis (75:24).

The tendency not to prepose /-apodoses in HCs should be strongly apparent in extraposed HCs, since apodosis-protasis order is the norm This is indeed the case Of 153 /+ reversals, 68 are also extraposed (c 44%); of the 25 /- reversals, a mere 3 (c. 12%) are also extraposed

Given the state of our knowledge about the Dutch HC system, it

is conceivable the differences between extraposed HCs and the rest are such that /- apodoses are excluded on grounds other than clause order. Certainly, the behaviour of extraposed HCs is different from other HCs in some respects. We have mentioned that the order *apodosis-protasis* is the norm for extraposed HCs; furthermore, unlike ordinary HCs, extraposed HCs do not permit *dan* ('then') in the apodosis when this follows the protasis (*Als je zou kunnen komen dan zou het leuk zijn).

However, even if we leave extraposed HCs out of consideration, the same trend against preposed /- apodoses remains. Approximately 27% of /+ apodoses precede their protases as against c. 14.5% of the /- apodoses.

10. Comrie (1986) notes that there are other languages where the protasis *must* precede the apodosis, such as Turkish and Ngiyambaa. In Dutch, conditional-temporal sentences of the type *krijgt-ie de bal, scoort-ie* ('if he gets the bal, he scores') where there is no explicit subordinator marking conditionality, the first clause must be interpreted as the protasis. For a counterexample, see Comrie (1986:97). Haiman (1986:222) notes that apparent exceptions in English of the type *You're gonna kill yourself, you keep driving like that*, where the protasis follows the protasis, are also intonationally different from sentences where the protasis comes first.

11. The subjunctive is considered formal in any case and may be giving way to the periphrastic conditional in general. This does not alter the validity of the intuitions expressed here, however (Jürgen Meisel, personal communication). Norbert Dittmar (personal communication) adds that *regnete* in *Wenn es regnete* - *if it rained*, while correct, is more likely to be restricted to written language. He also adds that preference for the subjunctive or the periphrastic conditional with *würden*, e.g. in cases where subjunctive and preterite do not coincide formally, may show regional variation.

12. Shana Poplack (personal communication) informs me that forms like *j'avais le temps, je t'expliquerais* are produced by French-Canadians in addition to double conditionals.

13. Whether 'd is would or had is controversial. For discussion, see Visser (1973:2424) and Fillmore (1985).

Note in this respect the following quotation from James Joyce's Dubliners (Penguin Books, 1965:64).

> 'He had a notion he was being had. He could imagine his friends talking of the affair and laughing. She was a little vulgar· sometimes she said: "I seen him" and "If I had've known". But what would grammar matter if he really loved her?'

14. In addition to those we have already mentioned (e.g. German, Dutch, Polish, Hebrew, etc.), we could also mention Russian, Latvian (Comrie, 1986), Rumanian (Mallinson, 1986·75-6), all of which may have conditional tenses in both clauses, and Ngiyambaa, an Australian language (Comrie, 1986). Haiman (1986:219-20) states that 'many languages' (in addition to those just mentioned) have symmetrical verb forms in the two clauses of a HC. He names the following languages: Cebuano, certain New Guinea languages (Gende, Kobon, Daga, Maring), Pitta-Pitta (Australia) and Hausa.

15. For a relevant example, see Lavandera, 1975.

16. There are 196 Dutch -/- HCs in the experimental corpus in Wekker et al. (1982). C. 93% of these are past time. There are only 25 L2 -/- structures, of which 72% are past

8 IF AT FIRST YOU DO SUCCEED [1]

8 0 Introduction

This chapter is a first attempt to deal with the concept of U-shaped behavioural development as it applies to second language learning, and in particular as it applies to the acquisition of L2s typologically similar to the L1, in this case English and German as acquired by Dutch speakers The term "U-shaped behaviour" is used here to describe systematic linguistic behaviour over time as realized in three distinct stages The first stage is characterized by performance on the part of learners (child or adult, L1 or L2) in some limited linguistic domain, which is error-free, that is, accords with the target norm (i e the adult language or the L2) The second stage is characterized by performance which is now deviant in terms of the target, and hence differs from performance in Stage One. The third stage marks a return to performance which matches the norm, as was the case in Stage One U-shaped behaviour thus refers to this tripartite sequence, where, in Stage Two, acquirers seem, to use Karmiloff-Smith's phrase, "to go beyond success" (Karmiloff-Smith, 1984) This tripartite sequence is shown in diagrammatic form in Figure 1

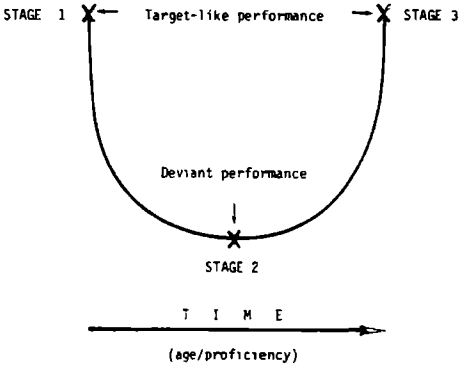

Fig 1 U-shaped behavior

8.1 U-shaped behaviour in first language acquisition

Let us begin with a well-known example Ervin (1964) reports how children, having apparently mastered English irregular past-tense morphology (*came*, *went*, *broke*), proceed to partially supplant these forms with regularised, and thus deviant, past forms (*comed*, *goed*, *breaked*) These new forms are themselves supplanted by the irregular forms already evident in Stage One, and finally disappear I will argue that the appearance of deviant forms in Stage Two should not be seen as evidence of attrition in linguistic competence, but as a 'cognitive advance' (Strauss and Stein, 1978), compatible with the notion that the acquirer has gone 'beyond success'

Although U-shaped curves in linguistic behaviour imply clearly discernible stages, they do not imply, as has already been suggested, that performance in respect of a given linguistic feature has to be categoric Maratsos (1979 313) indicates that deviant forms may co-exist with earlier correct ones, but the important issue is not variability in behaviour but rather the time of onset and disappearance of these innovative forms, as well as their nature [2] Researchers unfortunate enough to sample data only at Stages One and Three would get the false impression that a final state of acquisition had been reached, and maintained, from the earlier time of sampling

Bowerman's analysis of the causative verbs produced by her children (Bowerman, 1974, 1976, 1982a) provides evidence of cognitive advance despite apparent linguistic decline At Stage One, both causatives and non-causatives were used correctly, e g *Bring it*, *stay here*, *doll falls* At Stage Two, these children produce causative forms and begin to regularise suppletive verb pairs, e g *I m going to fall this on her* (for *I m going to drop this on her*) and *I came it closer*) for *I brought it closer*) In other words, some non-causative verbs were being assigned optional causative status in the child's grammar on a par with such adult forms as *open*, *close*, and *break*, which can be both causative and non-causative (*He opened the door*, *The door opened*) It must be assumed that such highly frequent verbs as *open* or *close* serve as

the models on which subsequent overextensions of causative meanings are based. The child appears to be sensitive to the implicit existence of a semantic feature [cause] contained in verbs like *open* and *close*. Consequently a number of new causatives are created, identical in form to the non-causative, intransitive verbs from which they are derived. These new uses of *come* and *fall* do not, however, lead to the extinction of *bring* and *drop* Evidence of this process of semantic analysis of [cause] is to be found in the presence of novel explicit periphrastic causatives with *make* immediately prior to the appearance of the causative uses of *come* and *fall*. Such forms argue strongly for the child having semantically decomposed such verbs as *close* into verb + [cause], hence forms like *I made that fall down* (Bowerman, 1982a·119)

Similar phenomena are reported by Bowerman (1982b) in the acquisition of 'reversative' verbs prefixed by *un-* Such verbs indicate 'processes in reverse' such as *untie*, *undress*, *uncover* Bowerman's children began by using adult forms such as *untangle*, *unfasten*, and *unbuckle* more or less appropriately At Stage Two, extensions to novel verbs began, showing that the elusive 'reversative' meaning of *un-* had been abstracted from its compounds and was being used in novel cases from which there would be no precedent in the adult input, e.g *unstraight* (= *bend*), *unshorten* (= *lengthen*), *unsprinkle* (= *stop something from sprinkling*), and crucially, even to verbs which already have reversative meaning, e.g. *unseparate* (= *separate*). Once again, these data may be analysed within the framework presented here initial reversatives are learned holistically, then the specific meaning carried by *un-* is distinguished, resulting in the creation of a number of novel forms; finally true adult competence is acquired as the child learns exactly which verbs may be prefixed by reversative *un-*

In her study of the language development of French children aged between 3 and 11, Karmiloff-Smith (1979) notes that after apparently having accepted that a single linguistic form can have more than one semantic function (Stage One) children may in certain circumstances insist on an isomorphism between form and meaning At Stage One, the successful stage, Karmiloff-Smith claims that these functions will have been treated as if they were expressed by separate (though

superficially identical) forms, but as the child develops linguistically so he or she proceeds as if the relationship were indeed one form to many functions The result of this discovery is that children tend to differentiate between the meanings they have teased out from the single form, at the same time creating new and distinct (but linguistically related) surface forms Karmiloff-Smith shows how French children initially postpose colour adjectives correctly, e g *La voiture jaune* (the yellow car), but at stage 2 distinguish between the descriptor and determiner functions of these adjectives, as dictated by the requirements of the relevant discourse Thus *La voiture jaune* is used to refer to the yellowness of the car, but a new construction with preposed adjective + partitive, *La jaune de voiture*, is used when it is important to contrast the yellow car with an array of cars of different colours This nontarget form later disappears, indicating the arrival of Stage Three

Another example, from Karmiloff-Smith (1979), of a stage of differentiation made by the child but not required by the adult grammar concerns *le/la même* (the same) In French, *le/la même* can mean both *the same one* and *of the same kind*, just as *same* can in English (*I have the same book you were holding just now* and *I bought the same book*) Initially, Karmiloff-Smith's subjects used *le/la même* to cover both the 'token' and the 'type' meanings At the age of 6, however, *le/la même* occasionally began to be used primarily for the sense of 'strict identity' and a new ungrammatical form arose, *un/une de même*, to indicate 'of the same kind' (*J ai une de même de vaches chez moi* or *La même de vache*) At Stage Three, from the ages of roughly 8 to 12, all the redundant markers and forms of questionable grammaticality gradually disappear

8 2 U-shaped behaviour in second language acquisition

There has been no explicit discussion of U-shaped behaviour in second language acquisition that I am aware of One recent study which does report on such phenomena (without using the name 'U-shape') is Huebner (1983) In his study of a Hmong learner of English, he refers to 'backtracking' in the acquisition of the function of the definite article That is to say, his informant

used *da (the)* ungrammatically more frequently halfway through the study than in the initial stages.

Another study dealing with developmental data that could be described as U-shaped is Wode, Bahns, Bedey, and Frank (1978), who discuss the initial appearance of correct "premature forms" like *feet, sheep,* and *fish* (cf. the Ervin study above) which then give way to regularized plural forms. Also clearly relevant to U-shaped phenomena are those studies which deal with chunk learning, where the learners correctly and appropriately produce a number of utterances, which, from the observer's point of view, exhibit a level of grammatical complexity that is clearly beyond the learner's proficiency as evidenced by his output *in toto*. Such chunks represent the outcome of holistic learning, as is indicated by the appearance at later stages of less complex related structures that are more productive.

To take examples from Fillmore (1976), the children she studied made frequent use of wh-interrogative formulae even containing adult-like *do*-support and subj-aux inversion as appropriate, without showing any evidence of controlling the internal structure of these formulae, e.g.:

O.K. what's going on here?
How do you do this?
Now, where did I put it?

(p. 621)

Again, at later stages, we find wh-questions without *do*-support and subj-aux inversion:

Why you come to my baby?
Why you go to the other room?
But when it gonna be?

(p. 628)

Hakuta (1982) reports that the child he studied was able to produce utterances of the following kind:

I know how to do it
I know how to read this
I know how to make

Such structures reflect the formula I know how to + VP However, at a later stage, these more or less correct forms began to become less frequent in terms of the number of overall how-embeddings, gradually giving way to such deviant forms as:

First I got to write it and show how do you spell Vino
We only know how do you make it like that

Hakuta notes that the incidence of these correct forms was associated with the onset of "other uses of indirect wh-questions where forms were first used with subj-aux inversion". The real issue that has yet to be settled is whether the success formulas are themselves analyzed in Stage Two or whether they simply atrophy with the independent development of interim grammars (see Krashen and Scarcella, 1978; Peters, 1983 for discussion of these points)

8.3 Cross-linguistic influence and U-shaped behaviour

The three cases of U-shaped behaviour illustrated below are different in a number of respects from those we have so far discussed. Firstly, they concern adolescent and adult Dutch learners of typologically close foreign languages in instructional settings. Secondly, the data derive from cross-sectional experimental studies. Thirdly, the data demonstrate the critical role played by cross-linguistic influence in the manifestation of U-shaped behaviour. The three studies deal respectively with· 1) the transitive/intransitive verb break, 2) the marking of hypotheticalness in the protasis (if-clause) of conditionals, and 3) the acceptability of L1-like idiomatic expressions in the L2

a) U-shaped behaviour and the transitive/intransitive distinction

Kellerman (1979a) reported on learners' acceptances of the translation of intransitive breken in the Dutch sentence Het kopje brak (The cup broke), as compared to their acceptance of translations of transitive breken in Hij brak zijn been (He broke his leg) The learners in question were arranged in eight groups according to the number of years they had studied English, and ranged in age from 13 to 23+. From the age of roughly 18 onwards, all subjects had been full-time students of English at university

If one plots performance on one sentence against the other for each group (where performance is to be understood as 'number of times per group' broke was accepted as a correct translation for break), then it is clear that up to the age of 17 there is no appreciable difference in performance for the transitive and intransitive forms At 18, the last year at school, there is a marked change Groups now start treating the two breaks differently, with only the transitive break being accepted fairly close to the optimum level There is a clear drop in performance for intransitive break, bottoming out among the 20-year-olds, and rising again for the most advanced group There is a significant difference, statistically speaking, between the behaviour of the younger groups (12-17) and the older groups. The performance details are displayed in Fig 2, where one may clearly discern a U-shaped pattern in the performance of the more advanced learners. The problem is how to explain the fact that the more advanced learners appear to perform less well than the remaining groups.

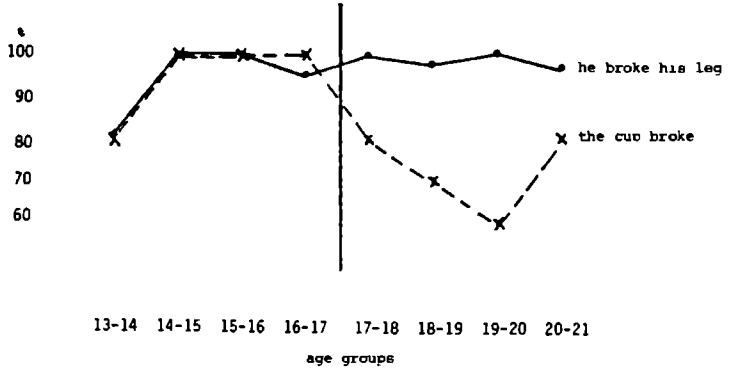

Fig. 2 Performance on transitive/intransitive *break*

It is my contention that this U-shaped curve for intransitive *break* is evidence for the development of sensitivity to a pragmatic distinction between causative and non-causative meanings of a single verb. Of course, such learners implicitly make the distinction as native speakers of their own language All the same, the apparent oddness of intransitive *break* in L2 English for more advanced learners is probably due to its lack of a specific animate agent in the test sentence. *Break* is seen primarily as a causative verb - cups as a rule do not break by themselves, and there has to be an overt agent somewhere, even if it is not mentioned in the same clause (cf. *The cup broke* and *John squeezed the cup until it broke* - further tests show that once it is embedded in contexts of the latter type, learners have no problem in accepting intransitive *break*, whatever their proficiency level. See Kellerman, 1983)

Sensitivity to this pragmatic distinction between causative and non-causative meanings is thus developmental as well as task-related These data, like those reported by Karmiloff-Smith above, provide insights into how learners seize upon and manipulate what appear to be important functional distinctions and attempt to give them distinct surface form (or reject forms where these distinctions are not made). The motivation for doing so must be internally generated, and cannot be related to the form of the L2 input. Younger learners, who have had less instruction, seem to be unconcerned about these subtle semantic and pragmatic distinctions. Probably they are more surfacy in their concerns, worrying more about problems of relexification. This means that their judgements give the false impression of being superior to those of more proficient learners (For a fuller discussion, see Kellerman, 1983, and Sharwood Smith and Kellerman, in press.)

b) U-shaped behaviour and hypothetical conditionals

The second case concerns the structure of conditionals in Dutch and English produced by Dutch learners. Dutch allows a modal auxiliary + infinitive in both protasis (the if-clause) and apodosis (the consequent clause), i.e. structures of the type

Als het zou regenen, zouden wij thuis blijven
If it would rain would we house remain

and also (modal) past verb forms in either or both clauses

Als hij kwam, zouden wij uitgaan
If he came would we go out

identical to the standard English target. There seem to be subtle meaning differences between these various structures having to do with the potentialis/irrealis distinction, the past tense in nonpast hypotheticals being counterfactual, the modal auxiliary signifying either hypothetical or counterfactual, depending on context

A characteristic and persistent "error" made by Dutch learners is to produce sentences of the kind *If it would rain, we would go out (but only rarely of the kind *If it rained, we went out or If it would rain, we went out, the translations of which are possible Dutch structures). The interesting question is why this should be so, given that there is a highly frequent Dutch analogue of the target English structure. Our research (Kellerman and Wekker, 1982, Klein, 1982) suggests that school-age learners may well produce relatively more target forms in English than do more proficient university learners. Once again it appears as if the least and the most proficient learners perform best. And once again such findings are deceptive. A closer look reveals that the younger learners are actually producing a range of Dutch-based structures in their English (such as modal past tenses in either protasis, apodosis, or both, or the auxiliary model zouden + infinitive in either or both clauses). This brings with it a number of nontargetlike structures, but also a number of targetlike ones (given the frequency of the latter structures in Dutch). At a later stage, learners restrict themselves very largely to just two structures, namely the targetlike one with a modal past verb in the protasis, and the structure with would in both protasis and apodosis. It is the latter which predominates in the last year at school and the first two years of college, and in some speakers it is never eradicated

As in our previous case, Stage Two goes beyond success The high incidence of conditional sentences with would in both clauses relates not to the sensitization of the learner to the notion of hypotheticalness but to the realization that what is morphologically a past tense does not have prototypically past tense meaning in

counterfactual sentences, thus the apparent desire in Stage Two to mark the distinction by formal means. Dutch provides a means of expressing hypotheticalness via a salient free morpheme equivalent in meaning to English would. At the same time the availability of this free morpheme allows the learner to circumvent the ambiguity of the past tense in English. The subtle irrealis meaning of the past tense in Dutch is thus sacrificed to the overt expression of hypotheticalness in English, as it may also be in Dutch, where even in strongly irrealis contexts, would equivalents in both protasis and apodosis are perfectly permissible.

It is relevant to note here that, in Dutch, in preposed apodoses, there is a tendency not to use the past tense and to favour the modal auxiliary, as is borne out by corpus data and acceptability judgements. This may be due to the momentary ambiguity of the past tense in the absence of a specific marker of conditionality like if or its Dutch equivalent als (Comrie, 1986) The structure of Dutch, plus the strong formal similarities between Dutch and English, permit advanced Dutch learners to make a distinction in English which is clearly important to them, even though it is not required by the target language.

An alternative hypothesis, particularly relevant to the language situation in the Netherlands, where English exerts a pervasive influence through the media (no foreign language films or television programs are ever dubbed), is that learners are exposed outside classroom time to forms of American English which do permit would in the protasis, i.e through the media and via the lyrics of the indubitably popular pop and folk songs. In other words, learners are just responding to the presence of would in the input, where its salience is no doubt enhanced by the existence of a parallel presence in Dutch

This argument raises a number of questions. First, just how frequent is the double would conditional in American English, and second, why should it be more salient than the standard (target) form, since this too has an analogue in Dutch? But the most critical argument against the presence of American would in the protasis being significant is the fact that Dutch learners of French chronically use the conditional tense in the si clauses of

hypotheticals where French requires an imperfect It would be very difficult to claim that this was due to the influence of nonstandard varieties of French in the input (unless we accord some special role to "junky" input from other learners, which argument is circular in any case).

c) U-shaped behaviour and idiomatic expressions

My third case concerns Dutch learners of German at university level, and their treatment of German idiomatic expressions. Jordens (1977) showed that second year learners tended to reject Dutch-like idiomatic expressions in German (whether correct or not) while first year learners were relatively accepting. Third year students showed signs of being able to distinguish those that were possible in German from those that were specific to Dutch. Jordens' Stage Two would seem to indicate that the learner becomes sensitive to the mismatch between the literal and the figurative meanings of idiomatic expressions, the more semantically opaque the expression in Dutch, the more likely its translation equivalent is to be rejected in the L2 Presumably at Stage One, learners do not concern themselves with this problem, working on the reasonable assumption that as Dutch and German are particularly close languages, they probably share idioms as well

In the three cases above, we may distinguish a Stage One where L1 structure appears to serve as a source for prediction about the form of the L2, not necessarily leading to error In Stage Two, the L1 declines as a source of prediction, even though the consequence may be error. Finally, in Stage Three learners acquire native speaker levels of competence in the linguistic domains involved In each case we may say that Stage One represents a structurally holistic phase of lexical exchange, plus the application of a number of local grammatical rules Learners at this stage do not appear to be sensitive to the fact that such abstract semantic notions as hypotheticalness, causality and metaphor are not given unique formal representations in their own language. For Stage One learners, since these notions are handled in a particular way in Dutch, they will thus be handled in the same way in the learner's L2 If learners at Stage Two lose confidence, so to speak, in this early

assumption, this may lead to a concern that these notions should be explicitly expressed in the target language. Where Dutch (the L1) does not provide the means for doing this, compensatory means must be devised in the L2, and hence the change in the interim systems and the corresponding dip in performance described above Thus for break, a supremely transitive verb in Hopper and Thompson's classification (1980), learners seek an implicit agent in English or look for verbs which they think cannot have causative meanings For conditionals, learners restrict the use of the past tense and use a modal auxiliary as the exclusive marker of hypotheticalness In the case of idiomatic expressions, learners indicate their uneasiness with the fact that literal and figurative meanings reside in a single structure by tending to reject the latter meaning

I have proposed here that the L1 has a role to play in the manifestation of U-shaped behaviour in second language performance It can serve as a source of structural predictions about the target language, but if it fails to mark what for the learner become important semantic distinctions in surface form, then it may appear that the learner is backsliding in creating novel forms In fact the crucial element in this interpretation is the relationship between L1 and L2. If L1 fails to mark a particular distinction which L2 does actually mark, then Stage Two may be represented by an apparent increment in that the learner now starts to produce correctly what he formerly got wrong. In such cases there will be no recognisable Stage Three Thus whether U-shaped phenomena appear at all will depend on the relationship between L1 and L2 in a given linguistic domain, even though from a learner's point of view, the same processes of abstraction and differentiation are at work regardless. This is what makes the study of typologically related languages and how they are learned so worthwhile - we need to know why learners become dissatisfied with the possibilities of formal expression their own language offers them in the L2

8.4 Is there a role for input in Stage Two?

Stage One is to be seen as an input-oriented phase in which the learner's output is successful both from a linguistic and situational point of view This is true for first language acquisition as it is for second language acquisition However, in Stage Two, the learner now sets about to reorganise his mental representations of these structures into a system within the relevant linguistic subdomain. In this stage, input no longer plays the crucial role it did in Stage One, nor does reinforcement from the environment. As Karmiloff-Smith (1984) states ". .children ... ignore or violate external reality in pursuit of the organising whole". This may result in the explicit marking of semantically important categories in surface form, which in fact leads to utterances which now become deviant. Thus what we see in the data presented by Bowerman and Karmiloff-Smith is evidence for the systematization of disparate items in the child's output. The child discovers the meaning and distribution of the past tense morpheme {-ed} and the reversative prefix -un and overgeneralises them to new contexts. French children no longer accept that one form can cover more than one function and will instead seek to mark these differing functions in their output, even though there may be nothing in the input that constrains them to do so. Exactly the same may be said for Dutch learners of English and German in the classroom The pragmatic oddness of an isolated sentence The cup broke forces Stage Two learners to seek less exceptionable ways of saying the same thing in English; the ambiguity of the past tense in Dutch (modal and non-modal, past and non-past) encourages learners to mark hypotheticalness overtly via an explicit modal auxiliary in English, Dutch learners of German react against Dutch-like German idioms, while their less proficient peers are happier to accept them. Deviant Stage Two behaviour cannot be related to the specific form of the input learners receive, since the systems learners appear to develop are not present in the input. To quote Karmiloff-Smith (1984) again·

> "It is by the child's own spontaneous problem-solving activity on languages as a formal problem-space that language acquisition progresses. Thus, external pressures from mismatch to model or

adult corrections cannot be invoked to explain the acquisition process Although such negative feedback may play some role at (Stage One), the behaviour of (Stage Two) children shows that successful output is not sufficient Children go beyond success to reorganise and understand the implicit information carried in their earlier correct forms It is the positive feedback from a success criterion that generates subsequent reorganization These linguistic data illustrate the general trend for a representational tool which functions well procedurally to become subsequently an object of spontaneous cognitive attention per se Newly discovered distinctions are often marked externally to render them tangible before they can be integrated into a more powerful underlying representation"

8.5 Conclusions

In this chapter I have tried to show that language acquirers go beyond success in the development of target competence They do so by systematic reorganization of their own output motivated by analysis of language as a formal problem space, irrespective of whether they are children acquiring L1 or adults receiving formal instruction in the classroom Stage Two, the deviant phase, must be seen as a stage where input no longer serves to mediate between the learner and his developing grammar, but where the learner generates his own solutions to the formal aspects of the linguistic structures in question. Only in Stage Three does input again appear to play a role, and then only when the learner has fully systematised the material available to him in satisfactory fashion The phenomenon of U-shaped behaviour implies that once-off data collection is potentially a risky business, since apparently correct target-like performance may be taken as meaning the acquisition of full target competence. This may well be a misleading assumption - the underlying mental representations of structures in Stage One will be quite different from those in Stage Three although their manifestation in the form of utterances will be identical In the case of second language acquisition, the typological similarity of L1 and L2 will itself be crucial in determining whether such behaviour will appear, indicating the complex relationship apparent in the interplay between linguistic domain and psycholinguistic process

NOTES TO CHAPTER EIGHT

1 This is a slightly revised version of a chapter originally published in S. Gass and C. Madden (eds), 1985. *Input in Second Language Acquisition*. Rowley, MA: Newbury House Reproduced with permission.

2. Despite their superficial similarity, U-shaped behaviour and "backsliding" or "variable behaviour" are not terminological variants. While in itself a purely descriptive term with no magical properties, U-shaped behaviour is used by researchers in developmental psychology to refer to performance curves consisting of clearly defined, stable, systematic, and long-term phases (cf the introduction to Strauss, 1982) What such curves mean for a theory of development is of course the interesting question Thus while "backsliding" is generally used to refer to unstable oscillation between later-acquired (often targetlike) forms and earlier (nontarget) ones, due to any number of triggering causes, U-shaped performance, as it is generally discussed in the literature, deals with movement through time from a targetlike phase to a nontargetlike one, and then back again, and represents changes in competence (Sharwood Smith and Kellerman, in press) If one must draw the parallel, it would be more appropriate to call such behaviour "forwardsliding".

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ASPECTEN VAN OVERDRAAGBAARHEID IN DE TWEEDE-TAALVERWERVING

Deze dissertatie bestaat uit een verzameling van zes artikelen en twee inleidende hoofdstukken die alle gewijd zijn aan het onderwerp van de kruiselingse beinvloeding en overdraagbaarheid in de tweede-taalverwerving Het onderzoek richt zich op de studie van de restricties in de beschikbaarheid van elementen in de eerste taal voor overdracht naar de tweede taal In het byzonder wordt een poging gedaan om antwoord te geven op de vraag naar de reden van het verschijnsel dat, zelfs bij typologisch nauw verwante talen zoals Nederlands en Engels, sommige eigenschappen van de eerste taal vaker deel uitmaken van de tussentaal dan andere, ook al laat zich dit niet voorspellen op strikt structurele gronden

Het feit dat een structurele vergelijking niet tot accurate voorspellingen leidt ten aanzien van de vorm van de tussentaal is vanzelfsprekend al eerder aan de orde gesteld, met name met betrekking tot the gebreken in de Contrastive Analysis Hypothesis Afgezien van enkele informele opmerkingen in de literatuur die vermeldden dat leerders fouten maken zelfs als de eerste en tweede taal congruent zijn (zodat overdracht uitgesloten is), is er echter tot voor kort geen systematische poging ondernomen om een verklaring te geven voor het ontbreken van een eenvoudige relatie tussen structurele gelijkvormigheid en kruis-beinvloeding

In feite wordt er gesteld dat de beslissende factor bij het bepalen van wat er wel of niet overdraagbaar is naar de tweede taal, het beeld is dat de leerder zelf heeft van de structuur van de eerste taal Dat wil zeggen, deze waargenomen structuur fungeert als de inperking van de mogelijke hypothesen over de overeenkomstige structuur van de T2 In deze opvatting over de kruiselingse beinvloeding, zullen aan sommige eerste-taal eigenschappen markeringswaarden worden toegekend, zodanig dat ze niet worden beschouwd als overdraagbaar naar een tweede taal Zulke markeringen kunnen al dan niet overeenkomen met de markeringen waartoe taalkundigen op zuiver theoretische grondslagen besluiten Het is onvermijdelijk dat een psycholinguistische benadering van markering de mogelijkheid met zich meebrengt dat verschillende individuen deze waarden anders zullen toekennen of dat deze waarden onderscheiden

effecten zullen hebben op de overdracht van de betreffende eigenschappen op de T2. Vandaar dat opvattingen ten aanzien van overdraagbaarheid zich kunnen ontwikkelen

Terwijl er wordt aangenomen, dat markering overdraagbaarheid bepaalt (dat wil zeggen overdracht in probabilistische zin), zal bovendien ook het beeld van de afstand tussen talen een rol spelen in de bepaling van wat er uiteindelijk wordt overgedragen In algemene zin wordt er een lans gebroken voor de stelling dat naarmate twee talen als meer verwant worden ervaren, des te waarschijnlijker overdracht zal plaatsvinden Dergelijke beelden zullen van taal tot taal verschillen en ook afhangen van het taalkundige bewustzijn en de graad van taalbeheersing van de leerder.

Na twee inleidende hoofdstukken, waarvan het eerste een overzicht geeft van het onderzoek dat er naar overdracht gedaan is, en het tweede specifiek ingaat op de theoretische en methodologische aspecten van mijn eigen werk, volgen er zes hoofdstukken die in meer of mindere mate gewijd zijn aan het onderzoeken van de aard van overdraagbaarheid in verschillende taalkundige gebieden Hoofdstuk Drie houdt zich bezig met een methodologie om tot een uitbreiding te komen van de database die leerders normaliter verschaffen door middel van elicitatieprocedures die retrospectie vereisen Argumenten worden naar voren gebracht om retrospectieve data te gebruiken als een heuristiek in de analyse van de zich ontwikkelende tussentaal. Hoofdstuk Vier gaat over idiomatische uitdrukkingen en laat zien dat die over het algemeen niet worden beschouwd als overdraagbaar naar de tweede taal, zelfs wanneer de tweede taal verwante vormen heeft Hoofdstuk Vijf behandelt de overdraagbaarheid van de betekenissen van een polysemisch werkwoord, breken, naar het Engelse equivalent ervan, break Er wordt aangetoond, dat een multi-dimensionale schaalverdeling van de oordelen van leerders met Nederlands als moedertaal aangaande de semantische overeenkomst tussen de verschillende betekenissen van breken een dimensie liet zien, die in hoge mate correleerde met oordelen aangaande overdraagbaarheid van Nederlanders die Engels leerden, onafhankelijk van hun taalbeheersing Deze dimensie, die geen betrekking had op concreet-abstract, leek de betekenissen te

rangschikken naar hun verwantschap met een proto-typische betekenis van breken.

Hoofdstuk Zes behandelt de concrete metaforische uitbreidingen van twee lichaamsdeel-termen, oog en head (hoofd/kop) Hier wordt aangetoond, dat de oordelen van leerders met Nederlands als moedertaal ten aanzien van de betekenisfrequentie en de semantische overeenkomst met de oorspronkelijke lichaamsdeel-term betekenis, na een geeigende statistische verwerking zouden kunnen dienen als een redelijk accurate voorspeller aangaande de mate waarin leerders de overdracht accepteren van metaforische betekenissen naar de overeenkomstige lichaamsdeel-term in de T2

Hoofdstuk Zeven is een bespreking van een karakteristieke fout die leerders van de Engelse taal met Nederlands als moedertaal in de morpho-syntaxis van hypothetische voorwaardelijke zinnen maken, dat wil zeggen het invoeren van would in de protasis waar het Engels een verleden tijd vereist Terwijl de voor de hand liggende verklaring van deze fout ligt in de overdracht van een equivalente struktuur vanuit het Nederlands zouden, lijkt experimenteel bewijsmateriaal te suggereren dat het verhaal niet zo eenvoudig is als het eruit ziet Met name lijkt het waarschijnlijk dat leerders niet in staat zijn gebruik te maken van de strukturele overeenkomsten van de Engelse en Nederlandse morpho-syntaxis in de protasis, omdat, evenals in Engels, in het Nederlands de verleden modaal tijd wordt gebruikt In het geval van protases die niet in de verleden tijd staan, betekent dit dat de verleden tijd geen werkelijkheid en ook geen tijdsaanduiding impliceert Voor degene die Engels leert betekent dit dat deze gemarkeerde betekenis niet overgedragen zal worden, en dat het ook moeilijk is het Engelse equivalent aan te leren. Derhalve zoeken leerders met Nederlands als moedertaal naar andere oplossingen, met name naar oplossingen met would

Hoofdstuk Acht gaat over het fenomeen van het U-vormige gedrag Hier is sprake van als leerders lijken over te gaan van een stadium dat wordt gekenmerkt door correct taalgebruik op een bepaald gebied, naar een stadium gekenmerkt door incorrect taalgebruik Daarop volgt een derde stadium waarin het taalgebruik weer correct is Afgezien van de methodologische betekenis van dit fenomeen dat, naar men zegt, typisch is voor het aanleren van de moedertaal, wordt er

geopperd dat - met betrekking tot de gebieden die identiek zijn in T1 en T2 - U-vormige gedragscurven laten zien dat het in het eerste stadium een kwestie is van overdracht, en daarom van toevallige correctheid, terwijl het tweede een stadium van reorganisatie is en het derde een stadium van werkelijke kennis van de T2 Het bestaan van het incorrecte tweede stadium is daarom van essentieel belang in een poging om overdracht van echte kennis te onderscheiden

Samenvattend kunnen we zeggen dat er in studies over het verwerven van een tweede taal weinig aandacht is geschonken aan de lexico-semantiek De studies waar het in deze dissertatie om gaat, vormen een van de weinige systematische pogingen om het aanleren van de woordenschat te behandelen Ze laten zien dat structurele overeenkomst niet voldoende grond biedt om overdracht te kunnen voorspellen, zonder dat er een bepaald concept wordt ontwikkeld dat betrekking heeft op restricties Dat er restricties bestaan blijkt uit het feit dat de psycholinguistisch gemarkeerde betekenissen van lexicale elementen als minder overdraagbaar worden beschouwd dan die van de meer proto-typische elementen Daarom lijkt algemene typologische verwantschap de rangschikking van de overdraagbaarheid van betekenissen niet te beinvloeden, ook al kan een dergelijke verwantschap overdracht bevorderen Zelfs in die gevallen waarin de T1 en T2 identiek zijn, houdt de leerder er duidelijke ideeen op na over hetgeen al dan niet deel uit zou moeten maken van de T2

ASPECTS OF TRANSFERABILITY IN SECOND LANGUAGE ACQUISITION

This thesis brings together a collection of six articles and two introductory chapters devoted to the topic of crosslinguistic influence and transferability in second language acquisition The work is concerned with the exploration of constraints on what elements of the first language are available for transfer to the second language. In particular, an attempt is made to discover why it is that even between typologically very close languages such as Dutch and English some first language features are more likely to form part of the interlanguage than others, even though on strictly structural grounds one might not predict this to be the case

The fact that structural comparison does not lead to accurate predictions about the form of the interlanguage has of course been noted before, particularly with regard to the failings of the Contrastive Analysis Hypothesis. However, apart from some informal observations in the literature which have noted that learners produce errors even when the first and second languages are congruent (thus ruling out transfer), till recently no systematic attempt has been made to account for the fact that there is not necessarily a simple relationship between structural similarity and crosslinguistic influence.

In fact it is argued that it is the learner's perception of the structure of the first language itself which determines what is or is not available for transfer to the second language That is, this perceived structure acts as the constraining force on the source of hypotheses about the corresponding structure of the L2 In this view of crosslinguistic influence, some first language features will be assigned markedness values such that they will not be considered transferable to a second language Such markedness assignments may or may not correspond to the markedness assignments made by linguists on purely theoretical grounds Inevitably, a psycholinguistic notion of markedness brings with it the possibility that different individuals will assign these values differently or that such values will have differential effects on transfer of the relevant features to the L2 Hence perceptions of transferability may be developmental.

While markedness is assumed to determine transferability (i.e. transfer in probabilistic terms), perceptions of language distance will also play a role in determining what is finally transferred. Generally speaking, it is claimed that the closer two languages are perceived to be, the more likely transfer is to take place. Such perceptions will also vary according to the languages concerned and the linguistic awareness and proficiency of the learner.

After two introductory chapters, the first surveying some of the research that has been carried out on transfer, the second being a specific discussion on the theoretical and methodological issues involved in my own work, there follow six chapters devoted to a greater or lesser extent to the exploration of the nature of transferability in various linguistic domains. Chapter Three is concerned with a methodology for extending the data base that learners normally provide by means of elicitation procedures which involve retrospection. Retrospective data, it is argued, can be used as a heuristic in the analysis of the developing interlanguage Chapter Four deals with idiomatic expressions, and shows that these are generally not considered transferable to the second language even when the second language has cognate forms. Chapter Five is concerned with the transferability of the senses of a polysemous verb, *breken* to its English equivalent, *break* It is shown that multidimensional scaling of Dutch native speaker judgements of semantic similarity between the various senses of [illegible] revealed a dimension that correlated highly with transferability judgements made by Dutch learners of English, irrespective of their proficiency. This dimension, which was not [illegible], seemed to arrange senses according to their proximity to a prototypical sense of *breken*.

Chapter Six deals with the concrete metaphorical extensions of two body part terms, *oog* (eye) and *head* (hoofd/kop). Here it is shown that native speaker assessments of sense frequency and semantic similarity to the primary body part sense would, with appropriate statistical treatment, serve as a reasonably accurate predictor of the degree to which learners would accept the transfer of metaphorical senses to the equivalent body part term in the L2

Chapter Seven is a discussion of a characteristic error made by

Dutch learners of English in the morphosyntax of hypothetical conditional sentences, that is, the insertion of would in the protasis where English demands a past tense While the immediate explanation for this error would seem to lie in the transfer of an equivalent structure from Dutch zouden, experimental evidence would suggest that the story is not as simple as it looks For one thing, it seems likely that learners fail to capitalise on the on the structural similarities of English and Dutch morphosyntax in the protasis because in Dutch (as in English), the past tense is being used modally In the case of non-past protases this means that the past tense carries neither real nor past meaning For the learner of English this means that this marked meaning will not be transferred, nor the English equivalent easily learned Consequently, Dutch learners seek other solutions, typically involving would

Chapter Eight is concerned with the phenomenon of U-shaped behaviour This is said to occur when learners appear to move from a stage characterised by correct performance in some domain to a stage characterised by incorrect performance Then there follows a third stage in which performance is again correct Apart from the methodological significance of this phenomenon, which has been reported typically for first language acquisition, it is conjectured that in the case of those domains which are identical in L1 and L2, U-shaped behavioral curves demonstrate that the first stage is one of transfer and therefore accidental correctness, while the second represents a stage of reorganisation and the third represents real knowledge of the L2 The existence of the incorrect second stage is therefore critical in helping to distinguish transfer from real knowledge

In conclusion we may say that lexicosemantics has been little considered in studies of second language acquisition The studies reported here are among the few systematic attempts to deal with the acquisition of lexis They show that structural similarity is not sufficient ground for predicting transfer, without building in some notion of constraint That constraints exist is demonstrated in the fact that psycholinguistically marked senses of lexical items are considered less transferable than more prototypical ones Therefore

although general typological proximity may encourage transfer, it does not seem to affect the transferability ordering of senses Even where L1 and L2 are identical, the learner retains a strong sense of what should or should not be part of the L2

Curriculum Vitae

Eric Kellerman was born in London. After attending St Paul's, he took a one-year teaching appointment at King's House, Richmond, fully intending to enter medical school thereafter. In the event he stayed at King's House for five years before going to Brighton College of Education (University of Sussex), where he studied Education, French and Drama In 1971 he was awarded the degree of Bachelor of Education (Honours) In 1972 he obtained the degree of Master of Arts in Linguistic Science from Reading University From 1972-1974, he was a postgraduate research student in the Language Department at York University In 1974 he was appointed to the English Department, Nijmegen University, where, at the moment of writing, he still is

I Bickerton writes that:

> "There is a pecking order within disciplines just as there is in barnyards. In linguistics, the theoretical linguist rules the roost: it is he who provides the descriptive models which, after a time-lag of a few years, are applied to the description of natural languages by the working grammarians and phonologists on the next level down. Models that have been tried and found effective, or at least fashionable, on that level are then handed on, like second-hand clothing, to workers in the field of child language acquisition, and then, after another lapse of a year or two, they finally reach the second language acquisitionist, who is already well on the wrong side of the pure/applied line, and has only language teachers to peck at". (D. Bickerton, foreword to T. Huebner, *A Longitudinal Analysis of the Acquisition of English,* Ann Arbor: Karoma, 1983)

However, *pace* Bickerton, there is currently a New Modesty in second language acquisition research expressed in the frequent disavowal of a direct link between research findings and classroom implementation. For the moment, researchers have beaks only for each other.

II. To date, and with very few exceptions, the main contribution of UG to SLA research has been to show how the heaviest theoretical apparatus can be coupled to the flimsiest experimental method.

III. Even after 20 years of noisy rejection, the Contrastive Analysis Hypothesis (R. Lado, *Linguistics Across Cultures.* Ann Arbor: University of Michigan Press, 1957) is still the touchstone against which many hypotheses about grammatical development in a second language are evaluated.

IV. In contrast to their American counterparts, European researchers have always believed in the importance of the first language in second language acquisition.

V. It seems churlish to mark Dutch learners down for erroneously inserting *would* into the protasis of English hypothetical conditional sentences when they are clearly improving on the original.

VI. Research into variability in second language acquisition has rarely gone beyond description.

VII. De Milliano's Principle of Language Learning Economy ("When reading a text in a foreign language, always assume the word you don't know is a misprint") is fundamentally true.

VIII. Dutch lovers of the cinema should band together to stop film theatres from insisting on an intermission in the middle of the feature film.

IX. Hawkstone Park, in Shropshire, is physically the most exciting 18th. century example of the art of landscaping in England.

X. Given the prevailing economic uncertainty in Dutch universities and the low rates of pay being offered to 'assistenten-in-opleiding', the sooner the Anglo-American model of examining Ph.D candidates is adopted, the better.